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THE DRAYSON PROBLEM

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[A Companion to "The Ice Age Fully Explained," by R. A. Marriott]

THE DRAYSON PROBLEM

AN ASTRONOMICAL SURVEY OF THE WHOLE QUESTION, IN THE FORM OF A REPLY TO A RECENT ARTICLE IN THE JOURNAL OF THE BRITISH ASTRONOMICAL ASSOCIATION ENTITLED "THE DRAYSONIAN FALLACY,"

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BY

ALFRED H. BARLEY

WITH AN INTRODUCTION

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To experience we refer as the only ground of all physical enquiry. But before experience itself can be used with advantage, there is one preliminary step to make which depends wholly on ourselves ; it is the absolute dismissal and clearing the mind of all prejudice, from whatever source arising, and the determination to stand and fall by the result of a direct appeal to facts in the first instance, and of a strict logical deduction from them afterwards.

HERSCHEL : *On the Study of Natural Philosophy*, p. 80.

PROLOGUE.*

Drayson, Geology, and Pre-History.

In the literature of the day devoted to philosophical and scientific questions, perhaps no subject is more often touched upon in one way or another than the Ice Age, the cause of which has hitherto been quite obscure, and its date and duration the subject of very diverse opinions.

For this reason it has been difficult to find any holding ground in which to anchor, before taking an all-round survey of the many points of evidence. And thus all the bearings of the question remain open to dispute, even to the extent of denying that the effect of a considerable (12°) increase in the Obliquity of the Ecliptic would be to enlarge the polar ice caps, and thus to enable elevated centres of dispersion like Greenland, Labrador, Keewatin, and Scandinavia to thrust forward their ice sheets into the temperate zone.

Again, the very strong evidence of the "submerged forests" in establishing a rise of the sea due to the melting of the glacial accumulations, instead of carrying conviction, has not been given due weight, since critics have confused the reasoning by coupling this phenomenon with that of the "raised beaches" which have an altogether different interpretation. And this although it had satisfied Clement Reid, our greatest authority in this matter, who in the course of his examination before the Committee on Coast Erosion said that these forests might as well be due to a rise of the sea as to a submergence of the land, and who wrote privately, just before his death, to the effect that he was quite ready to accept the fact of the Ice Age passing away only 7,000 years ago.

But numerous as are the geologists in this country who are inclined to the same opinion, and notwithstanding the conclusions arrived at by their fellow-workers in North America and in Sweden,—where in the latter case the views expressed *accord exactly* with Drayson's theory,—they are really helpless without the assent of the testimony of astronomy to establish their findings on an unquestionable and permanent footing. And therefore Drayson's theory, or rather discovery, and the additional proofs of it here brought forward have a scientific importance as far reaching as any that has hitherto been made, giving a new direction of thought to many sciences and enlivening the dry bones of geology and pre-history with a fresh interest. There is a point in the succession of possible coincidences, beyond which further coincidence becomes incontrovertible proof; but even that point has been surpassed by the evidence now supplied. To procure it Mr. Barley undertook a long and extremely intricate task, which could not have been achieved without great pertinacity, as well as considerable faith in the inherent *probability* of the movement asserted by Drayson to exist

* By the author of "The Ice Age Fully Explained."

Like all great truths it is simple to understand. So, also, are the very human processes of thought by which an initial error or partial statement of truth has in course of time become an adamantine dogma, which it has seemed almost sacrilegious to impugn, but which will at length have to be consigned to its due place amongst the defunct doctrines of a bygone age.

Besides being a matter of general interest to all mankind, there is scarcely any recognised branch of physical or historical science that will not receive a measure of enlightenment, when Drayson's discovery is established. To geologists, as also to astronomers, it should be an inspiriting revelation and a means of clearing away much lumber of theory, and for starting afresh; while pre-historians, with the growing accumulations of their material, without any guide to a definite chronology, even for the Neolithic Age, can now build more firmly on the *data* available.

"*The judgment hereof we referre to those who are most able to judge of these Arts. For seeing it is the fashion of many who would be thought to know much, that every where making ostentation of words and outward termes of Arts, they become a wonder to the ignorant, but a derision to those that are Masters of those Arts: we hope that our Labours shall have a contrarie successe, which is, that they may arrest the judgment of every one who is best vers'd in every particular Art; and be undervalued by the rest.*"

DE AUG. SCIENT., *vi*, 1: (1640).

[*This Introduction is intended for those unfamiliar with the astronomical side of the matter.*]

INTRODUCTION.

In approaching an unfamiliar subject, it is best to proceed from the known to the unknown. We will begin with the Cardinal Points of the year.

Everybody knows that the sun rises higher in the heavens during the summer than during the winter months. If we measure the daily shadows of a flagstaff or telegraph pole, we shall find the noon shadow on Midsummer Day (June 21) shorter than any other during the year. On the other hand, the longest such shadow is that cast six months later, at noon on Midwinter Day (December 21): the shorter the shadow the higher the sun, and the longer the shadow the lower the sun.

Everybody knows these facts, but not everybody realises all the interest that attaches to them. Not all people for instance are aware that for a few days before and after Midsummer Day, the noon shadows hardly differ at all from those of Midsummer Day itself, indicating that the sun reaches almost exactly the same point in the sky at each successive noon,—so that after mounting higher and higher each day for six months he seems for a few days to *stand still* before commencing his southernward *return*. These words are italicised because they furnish the explanation of two familiar words of which probably very few even well-educated people can give the real meaning, namely "solstice," and "tropic." The first is derived from the Latin *sol*, the sun, and *statio*, a standing (*c.f.* "armistice"). The second is derived from the Greek *trepein*, to turn; and it is perhaps characteristic of the English language that it should retain these two words, and use either indifferently to denote that phenomenon in which the Roman mind saw primarily a *standing still*, and the Greek a *turning back*.

As the same "standing still" occurs at midwinter when the Sun reaches his lowest midday position, these two points in the heavens are termed the Summer and Winter Solstices or tropics, respectively known as the Tropic of Cancer and the Tropic of Capricorn,—the geographical use of the two latter terms being due to the fact that places so situated have the mid-summer (or midwinter) sun exactly in their zenith at noon.

Some readers may incline to think these items unnecessarily elementary, but they will be mistaken. For it is in the literal sense of the word the *elements* of astronomy, with which we have to do.

Returning to our shadows, there is more that may be learnt from them. Our daily measurement of the noon shadow lengths would show us that while in the middle of Summer and the middle of Winter these shadows slowly reach to their shortest and their longest, varying little if at all from day to day, yet in Spring and Autumn they vary quite noticeably. Indeed if one had a building as high as St. Paul's to measure one's shadows from, one would notice a difference of six feet or more from day to day.

So that if we were to take the trouble to make very careful comparisons, day by day, the date when the maximum change takes place, first in spring and again in autumn, could be determined. These two dates are the

equinoxes, vernal and autumnal, the word "equi-nox" conveying the fact that the night (*nox*) is then equal to the day. On these dates the Sun passes across the equator or "crosses the line," northward (March 21) and southward (September 22), and day and night are equal all over the globe.

It is thus possible to locate, by no more elaborate means than the observation of the noon-tide shadows of some high object, the four important points of the year with which we associate the ideas of Summer and Winter, Autumn and Spring, technically known as the solstices and the equinoxes. And that this was the method used by the ancients is shown by Mr. Cotsworth in his book on the calendar.*

In making this little survey of the Cardinal Points of the year we have in fact reviewed the earliest eras of astronomy. The determination of these points of the year, and of the exact number of days intervening between them, must have been for long ages one of the most important functions of the learned men of those early times. And thus originated that astronomical priestly caste whose jealous reservation of its privilege of knowledge has not altogether died out to-day.

Determination of Latitudes.

When from a pastoral or agricultural stage the nations passed into the maritime and commercial, necessitating journeys over long distances by sea or by land, a new use for astronomy began to declare itself. The spherical shape of the earth, known far earlier than is commonly thought, required that men should have some idea of the latitudes of different places, even in the same kingdom, but more particularly in distant lands. For this purpose shadow observations showed themselves to be most useful, both for their simplicity and for their accuracy, as accuracy was then understood.

In referring just now to the length of the midsummer (noon) shadow no mention was made of the latitude of the place at which the observation is made. Yet this is an essential factor, as one must immediately perceive if one supposes such an observation to be simultaneously made at London and Bordeaux, which is some 7° south of London. Whatever the height of the sun at London, it would be a full 7° higher at Bordeaux.

As soon as we take in the significance of this fact we can easily see that if we knew the latitude of one place, and the height of the midsummer sun there, we could at once ascertain the latitude of any other place by finding the length of the midsummer shadow there. Or rather, to express the idea more accurately, the *proportion* borne by the length of the shadow to the height of the rod or other object which casts it. For, having this, we can construct a right-angled triangle whose base represents the shadow and perpendicular the rod, and from this determine the angle facing it—which of course gives the sun's height or "altitude."

Cleared of details, this is just what the ancients did; it was *the* method of finding the latitude of a place. And that it is a good one the writer can vouch, from some recent tests, whereby he was able after a few trials to obtain the latitude of his own house correct to within a few minutes—that is to say,

* See also Narrien's *History of Astronomy*, pp. 57, 58.

within a few miles—with the prospect of greater accuracy after further practice.

The Obliquity of the Ecliptic.

It will be noticed that in order to obtain the latitude of a place correctly by this method, it is important to know exactly what is the greatest height reached by the midsummer sun, technically known as the sun's maximum northern "declination" by which it de-clines or slopes away from its middle or zero position just over the equator at the beginning of spring. This quantity at the present time is 23° 27′, but in former times it was more. *How much more, do you ask?*—There's the problem! For here we come upon a disputed point. According to orthodox astronomy, the answer is about $1\frac{1}{2}$ degrees, according to Drayson about 12 degrees—eight times as much. It is however agreed that since the commencement of the Christian era it has become less by about half a degree, and it is known to be decreasing at the present time at the rate of about half a second of arc annually.

The ecliptic is so called from the fact that it is that line in the heavens on which (or *very close* to which) the Moon must be if an eclipse is to take place; and when we think this out, we see that it must mean the apparent annual path of the sun in the heavens—for an eclipse cannot occur unless either the moon is just between the earth and the sun (solar eclipse), or the earth between the moon and the sun (lunar eclipse).*

Now the annual path of the Sun, as we can see from what has gone before, must be a circle in the heavens—a circle which passes across the equator at those points known as the vernal and autumnal equinoxes, and reaches its greatest distance away from the equator at the summer and winter solstices. It is not a circle parallel to the equator, like a parallel of latitude, but a circle *oblique* to the equator; and hence the term "obliquity" of the ecliptic.

Importance of the Question.

Whether or not the summer sun two or three thousand years ago came a little further north than it does to-day, may not seem a very important question to have to decide, until one reflects that the difference between the climates of, say, Bordeaux and Edinburgh depends upon this very fact; the fact, namely, that the midsummer sun rises about 12 degrees higher in the one place than in the other. Wherefore it immediately becomes a matter of interest to enquire whether the climate of familiar places may not have been vastly different in ancient times when the height of the midsummer sun (*and also*, be it remembered, the depth of the midwinter sun) was TWELVE DEGREES more than it is now.

It is clear that for one thing it would mean the extension of the Arctic circle by just that amount. The northern frigid zone would extend down so as to include places in the latitudes of Durham and Belfast!

One need not enlarge on this. Major Marriott in his interesting articles

* Another way of expressing the same idea would be to say that the "Ecliptic" is the orbit of the earth, for it comes to the same thing.

on "Warmer Winters" has explained the changes likely to occur under these conditions, and has summarised the evidence which exists to show that *such changes have in fact occurred.*

All that is needful, then, in this connection is to point out that Drayson's cycle of thirty-two thousand years offers us the phenomena of a Great Year —a "year" having its "spring," its "summer" (in which we now are), its "autumn" and its "winter," and that in this way it becomes a kind of GEOLOGICAL CLOCK by which to measure time.

These are the outstanding features of Drayson's discovery. Others remain, to be disclosed later.

The Issue.

The reader will enquire: "If Drayson says the obliquity was formerly 12° greater, whereas orthodox astronomy says it was never more than $1\frac{1}{2}$° greater, there must surely be some explanation of this difference. What is it?"

The explanation, in itself, is quite simple. But it is not possible to put it quite simply before the reader at this stage, for the reason that it involves certain unfamiliar and therefore, on that account, not-easily-dealt-with conceptions.

We will approach the matter by stages.

The Astronomers' Explanation.

Some hundred years or more ago, when geologists were seeking a cause for the Ice Age, and thought that a greatly increased obliquity would supply it, they addressed themselves to the leading astronomers of their day, asking if and when such an increased obliquity had existed. Laplace, the great mathematical astronomer, then in the height of his fame, undertook to reply. His answer was, that $1\frac{1}{2}$ degrees or thereabouts was the maximum variation which could occur from the present obliquity.

That settled it. Geologists, though dissatisfied, could of course do nothing. Exact science had spoken!

It may seem rather impertinent to speak in this way of the investigation of Laplace. The impertinency, however, in the etymological sense of the word is on *his* side. For the investigation which he made was, what was the possible variation of the plane of the earth's orbit with respect to the stellar celestial sphere; that is to say, how far it could vary from its then position.

Yet these were not the factors concerned. The relation which alone could decide the question was, not the relation between the earth's orbit and the orbits of the other planets, which was what he investigated, but, the relation between the *earth's orbit* and the *earth's axis.* For it will surely be evident from the simple facts which have been recited, that the "obliqueness" of the ecliptic to the equator depends upon the angle at which the earth's orbit (*i.e.*, the ecliptic) is inclined to the earth's axis or poles? The reader can see that for himself.

It seems incredible that a great astronomer could make such a mistake. It is even rather comic. But it is still more incredible that in this very investigation he should also have made a serious mathematical blunder which entirely vitiates his results, and that this blunder should moreover have remained undetected for more than a century. That is even more comic. Yet of this, too, the reader can assure himself. The proof will be found in its due place.

Enough to say here, that Laplace having spoken, his word was sufficient. No one cared to re-investigate the problem *de novo*. His calculations were repeated (*e.g.*, by Stockwell) without discovering his error—so easy is it to miss the wood when lost amid the trees!

"How did it happen, then, that Drayson discovered this error?" the reader will ask, and the answer may surprise him. Drayson did not discover it. In fact he died some years before its existence was suspected.

The error which he exposed was of a different character, its discovery was led up to in a very singular fashion, and it is not unlikely that some day, when the greatness of the issue underlying this problem shall have become appreciated, posthumous honour will be paid to the 'unknown student' whose acute remark led to the revealing of it, though Drayson was the first to avail himself of the hint, and to pursue the matter further, resulting in what succeeding generations will acclaim as the most important discovery that has been made in astronomy since the establishment of the Copernican system. But to explain properly how this "eye-opening" came about, a somewhat lengthy digression into an apparently unrelated subject must be made.

The Precession of the Equinoxes.

The few astronomical facts that have so far been mentioned relate to the Earth and the Sun only, they are not concerned with the Stars. It is now, however, necessary to take them into consideration.

Everyone knows that when we look out upon the midnight sky at Christmas time, we do not see the same constellations as in the summer, but quite different ones; and knows, too, that when winter time returns the same constellations will again be visible. This progression is quite regular, for if we watch any particular star, say Aldebaran or Sirius, we shall find that each night it comes to the meridian, or "souths," four minutes earlier than on the previous night,* until in course of time this happens before sunset, when the star can of course no longer be seen owing to the strong light of the sun, and we have to wait perhaps some months before it again becomes visible. It is from observations of this kind that the apparent annual movement of the sun among the stars—due actually to the motion of the earth in its orbit—has been deduced; and the position of the ecliptic among the stars has similarly been laid down by indirect means.

All this is doubtless familiar to the reader, as also the fact that after the lapse of an entire year the stars return exactly to their former position in the sky; so that if one looked out at 10 o'clock on a certain night and saw

* See *Whitaker's Almanac*, where the times of southing are given.

a star just emerging from behind a chimney-stack, then at 10 o'clock on the same day a year later one would see the same star emerging from the same chimney-stack.*

But here comes in a curious thing. If one were to wait 72 years and then repeat the experiment, one would find the star four minutes behind time! It would not emerge till 10.4 p.m. And the cause of this unpunctuality is what is termed "precession," a word which will be explained in due course.

The discovery of precession is said to have been made by Hipparchus about 120 B.C., on comparing the distance of the star Spica Virginis from the autumnal equinox with that recorded about a couple of centuries previously. He found it about two degrees further advanced, and his natural assumption that all the stars were affected in the same way was duly confirmed by further observations.

What concerns us most, however, is the manner in which this phenomenon was explained.

The Explanation.

In order to enter into the spirit of this explanation we need to visualise the fact of precession from the standpoint of day, instead of the standpoint of night, as we have done.

Imagine that it is midwinter day and that you are standing on some open space in the south of England looking up at the Sun. It is just noon, and you realise from what has already been said that if there were a hole right through the Sun and you could see some fixed star behind it, then if you were to repeat the observation just seventy-two years later, the star would not be shining through the Sun at all, but would be to the *left* of him, two full sun-breadths away, so as to "south" or come to the meridian four minutes after him.† The stars, then, although one cannot perceive it, are actually moving slowly round to the left—counter-clockwise.

Now to the explanation. The Sun that you are looking at is in the position of the Winter Solstice. The Summer Solstice therefore lies behind you, as much *below* the northern horizon as the Sun in front of you is *above* the southern horizon (about 15°). The meridian above you is the "solstitial colure," since it passes through the solstice and the pole of the heavens. And by taking a sweep with both arms from the Sun to the east and west points of the horizon—where now lie the Equinoxes—and continuing till they meet behind your back you will have swept out the exact position which the earth's orbit, or the Ecliptic, holds in the heavens at that moment.

This is important, because the situation you are now in enables you to picture quite clearly not only the Plane of the Ecliptic, but also the Pole of the Ecliptic.

* It is not necessary to trouble the reader with the slight qualification which this statement requires. It is substantially, not *literally*, correct.

† This gives the clue to the origin of the term "precession," for it shows that the sun (*i.e.*, the solstitial point) *precedes* or comes to the meridian four minutes earlier now than does the star which was its companion of seventy years ago. We might just as well say "the Precession of the Solstices," only having first been noticed in connection with the Equinoxes that gave the name.

Now the " pole " of a great circle of the sphere is that point which is 90°, or the fourth part of a circle, from every point on its circumference. Thus the zenith is the pole of the horizon, and the geographical pole is the pole of the equator.

The position of the Pole of the Ecliptic is now easily found. Imagine that as you are looking at the Sun, the horizon is gradually tilted up 15° until they touch : at the same time its ' pole ' the zenith would be tilted back 15° : what was formerly the *horizon* would now coincide with the *plane of the ecliptic*, and what was formerly the *zenith* would coincide with the *pole of the ecliptic*. Put more shortly, the plane of the ecliptic at the instant specified passes through the Sun's centre and the east and west points of the horizon : and its pole is as far from the zenith towards the north, as the sun is above the horizon.

The explanation which the ancient astronomers gave of precession, was that it was caused by a turning of the whole starry sphere round the poles of the ecliptic as an axis or centre. And we can see from what has gone before that this would completely account for the observed movement, which would in fact be identical with that of your left arm in its sweep from the Sun to the east-point. By this movement the stars would sweep past the solstitial colure, already mentioned, and the equinoctial colure, which is at right angles to it ; and hence, as Hipparchus found, the " longitudes " or distances of the stars from the latter would gradually increase.

This is the explanation of precession offered by the ancients. It has received the following modification. In former times, the planets and the sun were supposed to perform their orbits about the earth as a centre ; but from the time of Copernicus onward, the doctrine has been received that the sun is the true centre of all these orbits, including that of the earth. The Pole of the Ecliptic has thus been reduced to the comparatively insignificant position of being the centre of a planet's orbit, instead of as theretofore the centre of all the orbits including that of the sun. Accordingly, since it no longer seemed fitting to suppose that the whole starry sphere turned round the pole of the orbit of one small planet, an interpretation of the observed phenomena was secured by *inverting* the former explanation. That is to say, instead of supposing that the stars as a whole marched past the solstitial and equinoctial colures, they supposed that it was the colures which caused this appearance, by themselves moving in the opposite direction.

Now, as we have seen, the position of the solstitial colure depends upon the position of the pole of the ecliptic and the pole of the heavens ; so that if either of these move round the other the solstitial colure will change its position with respect to the stars, and their longitudes will increase. But it was simpler and easier to suppose that the pole of the heavens, *i.e.*, the prolongation of the earth's axis, moved round the pole of the ecliptic, rather than *vice versa ;* and this explanation was accordingly adopted. A little later Newton offered a superlatively ingenious explanation of the *cause* of this movement, which was unanimously accepted, and the problem was thence-forward regarded as solved and beyond the reach of discussion.

This was it. According to the gravitational hypothesis the earth, if originally a molten semi-fluid mass, must owing to its rotation be not

strictly spherical but spheroidal in form, bulging out somewhat at the equator. Newton accordingly decided in his own mind that this was the shape of the earth. Scientists generally were inclined to approve the idea, various geodetical measurements lent support to it, and in short it was adopted. Newton then conceived the idea that this bulge or protuberance at the equator might possibly provide a clue to the strange phenomenon of precession. He investigated the matter geometrically and satisfied himself that this was the true explanation. His demonstration was accepted and was acclaimed as a triumph of scientific insight, and, until Drayson wrote, had never once been questioned.

The movement that had thus been conceived of was that of circular motion round a centre. The stars, first, were supposed to perform this movement, the pole of the ecliptic (" E ") being the centre. Then, later, the stars being supposed at rest, and their motion being transferred to the pole of the heavens (" P "), it followed that the pole of the ecliptic came to be regarded as the centre of the polar motion. That was the doctrine taught, and implicitly believed in, centuries ago—and since, for Newton's explanation seemed to clinch the matter. And yet, although thus believed in and accepted as a fact, it seems that in the course of years *one* little thing had become overlooked ! What that was, we shall see directly ; but we have now come to the end of our long digression and can return to the point from which we departed (page xiii).

The Young Heckler.

Drayson was for some fifteen years professor at the Royal Military Academy at Woolwich,* lecturing on astronomy and military surveying to the young cadets there. It happened one day, that after he had delivered a lecture on the subject of precession, with some such explanation as has just been given, a young cadet enquired if the distance between the pole of the heavens and the pole of the ecliptic had always been the same.

" No," was the reply ; " It was formerly somewhat greater, and it is subjected to an annual diminution of about half a second, which is termed the Decrease in the Obliquity of the Ecliptic."

" Did you not say just now," pursued the enquirer, " that the precession of the equinoxes was caused by the Pole of the Heavens moving in a circle round the Pole of the Ecliptic ? "

" So it is."

" Then what is the centre of that circle ? "

" The centre ? I told you, the pole of the ecliptic."

" But you have just told us that the Pole of the Heavens is decreasing its distance from the Pole of the Ecliptic. How, then, can this be the centre of the circle in which the Pole of the Heavens is moving ? "

This was indeed a dilemma. Drayson answered honestly that he had never considered the matter, but had given the usual explanation according to the text-books. He said there did seem an apparent contradiction, and he promised to enquire further into the matter.

* He had also spent two years at Greenwich Observatory.

He did enquire further, and as a result of his questioning discovered that nobody had ever thought about the subject. It had simply been overlooked. Two thousand years previously the pole of the ecliptic had been supposed the centre of the observed motion, and this literally time-honoured supposition had become accepted, without examination, as a proved fact. It is indeed so regarded to-day.

Probably nothing in the whole history of science is likely to prove so illuminating to the student of group-psychology, as the growth and propagation of this extraordinary paradox. Long before the publication in 1687 of Newton's *Principia* even, it was suspected—indeed it was openly maintained by some—that the obliquity was less than in former times; and for the last two hundred years it has been definitely known as an ascertained fact. And yet in Drayson's time it was being taught (*it still is!*) that the Pole of the Ecliptic is the centre of the Polar Motion! Who could ever dream that when such an anomaly was pointed out, no notice would be taken, except to request the speaker to be silent and refrain from airing his ignorance? But so it was.

Yet can one say that the young cadet used the language of intemperance, or of exaggeration, when on Drayson reporting the official pronouncements in reply to his first enquiries, he said: "It seems to me, Sir, that to put those two explanations side by side, is all bosh. I may not know as much as the writers of text-books, yet I do know that the circumference of a circle does not decrease its distance from the centre."

What Drayson Discovered.

Drayson perceived that here was a matter for investigation. The pole of the ecliptic had been supposed the centre of this movement at a time when the obliquity—which is the same thing as the distance between the pole of the heavens (P) and the pole of the ecliptic (E)—was thought to be invariable. Then, when the decrease became known, the fact that this showed that E could not be the centre of motion of P, had been overlooked. The questions therefore arose: *Is the path of the pole a circle, or some other curve? If a circle, what is the centre of that circle?* Drayson set to work to investigate the matter.

Briefly he found that the course of the pole is a circle, a circle round a point distant six degrees from the pole of the ecliptic; and that this movement is caused by a slow rotation of the earth, in a sense opposite to the daily rotation, and about an axis fixed in space not as regards the earth but as regards the heavens,—the period of this rotation being about 32,000 years.

That is his discovery in a nutshell. A number of curious consequences follow, however, arising from the fact that it is impossible for an error in astronomical thought to be perpetuated for centuries without begetting sundry anomalies; and one of the most interesting of these consequences is the so-called "proper motions" falsely ascribed to the stars.

However, one thing at a time the reader may say. And the first and most important thing from the layman's point of view is the explanation

afforded of the date and duration of the last glaciation and the occurrence of former periods of glaciation,* with their intervening "genial" periods, to which may be added the forecast of a future ice age, a few thousands of years hence, as seemingly bound to occur.

Drayson accordingly hoped much from the light thrown by this discovery of his upon the Ice Age. He was disappointed, however. Leaders in geology in this country were then [the seventies] going through a phase of thought on the subject which made a comparatively *recent* date for the passing away of the Ice Age appear preposterous—since geologists in general paid little heed to the new idea then beginning to form in North America regarding the last period of ice invasion. So from geologists, at that time, he received no support.†

Astronomers rejected him *nem. con.*, then and thereafter. And in spite of his persistent efforts to eliminate a palpable paradox from astronomical doctrine he was regarded not as a friend but as a trespasser. He has been contemptuously termed a 'vague theorist,' and even—oh, irony!—a 'paradoxer' by some astronomers. The long labours of Admiral Sir A. de Horsey on his behalf have in consequence been pronounced 'pathetic,' while his co-worker has received (in some quarters) the *sobriquet* 'a geological Don Quixote.'

The discovery which he made is frequently alluded to as "Drayson's theory," but this is not correct. A theory may be plausible, or it may be far fetched; but it rests on *suppositions*. Drayson's discovery was the result of a geometrical investigation, and not only was the position of his centre of polar motion (*C*) derived from actual tests, but so also was his explanation of the movement, namely the Second Rotation as he termed it, proved by investigation.

To apply, then, to the conclusions derived from the result of this work the term "theory" is not only not correct, but not fair. We do not term the result of a geodetical survey a "theory," and why should we apply it to the result of an astronomical survey conducted on the same principles? Drayson had no theories, and might have said with Newton, *Hypotheses non fingo*. To a question as to how he accounted for the Second Rotation he said he did not profess to assign a cause for it, adding dryly, 'I do not even know what is the cause of the Daily Rotation. You see how ignorant I am.'

From which we see that Drayson, who was talented with many gifts in other paths of research, must also be accredited with that of humour.

Anent the Second Rotation it may be added that although this was regarded by astronomers generally as an altogether novel—and needless—conception, whereby to explain the phenomenon of precession, the soundness of the idea, and indeed its inevitability has already been *implicitly* recognised

* A "glaciation" is a pronounced lowering of the winter temperature in the arctic and temperate zones, enabling glaciers to form at elevated centres of ice dispersion, and to spread therefrom as ice sheets over certain areas of low elevation, conditioned by the configuration of the land and the amount of precipitation in the centres of dispersion. —R. A. Marriott.

† An exception was Thomas Belt, author of "A Naturalist in Nicaragua," etc., who warmly espoused the idea.

by no less an authority than Sir John Herschel. A reference to this is made in the Concluding Remarks.

To show that the question of the true polar motion cannot be simply brushed aside, as has been attempted by an astronomer in a correspondence on this subject which appeared in *Science Progress* a year or two ago and is further referred to later on, I append here the letter of "Draysonius," sent out with this year's Proceedings of the East Anglian Prehistoric Society, which quotes passages from various authors expressive of the conviction that the question of the Ice Age is interlocked with that of Drayson's discovery :—

THE GLACIAL EPOCH.

The Date and Duration of the Last Ice Period.

(*This inset is brought to the notice of the readers of this issue of the "Proceedings," not with the intention of endorsing the views therein stated, but out of respect to one of the chief founders of this Society, the late Dr. W. Allen Sturge, whose immense activities in pre-history are well known, and whose critical acumen led him to favour the acceptance of Drayson's contention as a* WORKING HYPOTHESIS *for the elucidation of the various ages to be assigned to flint cultures in East Anglia. Several discoveries since his death have done much to favour an unexpected extension of the stages of man's progress, and of the enormous antiquity to be assigned to primitive man, while certain facts on the astronomical side have so strengthened the position of Drayson's supporters as to make their claims a matter inviting serious attention.*)

Readers of these Proceedings will recall the paper read by Dr. W. Allen Sturge in Vol. I, part iii, 1913, containing arguments based on the recurrence of glacial periods, such as may be postulated from an acceptance of Drayson's discovery of the true motion of the Pole, and especially as to the chronology of the last one with reference to the Neolithic culture ; and many subscribers are further acquainted with the numerous corroborative evidences bearing on this question of glaciation furnished by geology and other sciences, as set forth in a recent pamphlet by Major Marriott.

This strong chain of evidence, however, laboured under the disadvantage that the connecting astronomical links had not received the hall mark of official astronomy, and therefore were considered unsound ; and it is only quite recently that the orthodox school have deigned to give their reasons for rejecting the astronomical evidence without investigation.

The objections brought against Drayson's conclusions have thus been displayed in all their weakness, and have given Mr. A. H. Barley an opportunity to combat them, phrase by phrase, by the production of mathematical proofs from several sources, which place the matter on an impregnable footing, such as no unprejudiced astronomer can fail to see establishes a claim for minute investigation.

Apart from these concise proofs, researches into archaeology and the orientation of ancient monuments mirror the same deductions from terrestrial sources, being corroborated by recent pronouncements on prehistoric climatology,* while providing a basis for many myths, especially in connection

* C. E. P. Brooks, F.G.S. : "The Evolution of Climate." Qu. Journal, Met. Soc., vol. xlvii, July 1921.

with Vedic and Avestic traditions, and enhancing the grandeur and utility of the Great Pyramid by giving it a new astronomical significance.

Until the exhumation of this discovery, which was first put forward in 1859, geologists have failed to get any nearer to the solution of the Ice Age question, since the days of Agassiz a century ago ; but it has now been revivified by independent research, giving a new impetus to geology and providing us with a clue of great importance to serve as a guide in prehistory. The following quotations from other sources show the inherent interest of the subject.

DRAYSONIUS.

"The glacial phases afford the student of ancient man a series of milestones to mark his journey into the past. Unfortunately we are not certain of the exact number of glacial phases, and, what is still more unfortunate, we are not yet in a position to offer a complete explanation of their occurrence and recurrence. The day will certainly come when their course, their duration, and their sequence will become common knowledge. At the present time the only explanation which answers the needs of those who are tracing man's history in recent deposits is that put forward by Major-General Drayson in 1888 ("The Earth's Past History": Chapman and Hall). . . . . His work and conclusions are being vigorously advocated by Major R. A. Marriott, D.S.O. ("The Change in the Climate": Marlborough & Co., London). The important point for us is this, that if Drayson's hypothesis is well founded, the last ice age reached its maximum about 13,000 years ago, a date which fits very well with the evidence brought forward by geologists."—*From "The Antiquity of Man," by Professor (Sir) Arthur Keith. (Williams & Norgate, London.)*

"That which I have put forward and claim to prove is not, as some writers have stated, 'a vague theory to account for the Ice Age,' but it is an accurate geometrical proof of a movement of the earth hitherto not quite correctly interpreted. To have invented a theory many years ago to account for an Ice Age having terminated 7,000 years ago, at a time when the geological world were agreed that 150,000 years had elapsed since it terminated, would have been an act of folly of which I was not likely to be guilty. I have no theories ; I merely base my conclusions on the accuracy of geometrical calculations."—*An article by Drayson in the "New Science Review" for July*, 1895, *entitled "The History of an Astronomical Discovery."*

"It is inconceivable to me that the Royal Astronomical Society has not enthusiastically taken up a subject which gives such strong evidence of its truth. To sift such a matter—a matter affecting the basis of all astronomical calculations—to the bottom is surely the first duty of a learned society which has the privilege of the prefix 'Royal.' I should have imagined—as I believe you did—that you had only to whisper '29° 25′ 47″' in order to put astronomers on the *qui vive* to elucidate a discovery of enthralling interest, which for a chaos of supposed proper motions of millions of stars substitutes a simple movement of the instrument (the earth), by which and from which all astronomical observations are made."—*From a letter of Admiral Sir Algernon de Horsey, K.C.B., to Drayson, dated 23rd April*, 1894.

"Something more is required from astronomers than the invocation of the great name of Newton in support of a circle with a phantom centre. This philosopher, when he made his great discovery of the mechanism of the Universe in the laws of motion, naturally applied them to the finding of a cause for Precession, but Newton was not confronted with geological research, and knew nothing of the decrease in the obliquity, nor the true conformation of the earth. Are his conclusions, then, never to be questioned ?

"We are not more capable certainly than the great minds of the past, only better equipped. Not to use these advantages will be to incur the reproach of the age of Galileo. Let the facts here outlined be faced and admission made that much remains to be investigated, and Drayson ere long will be assigned his place among the Immortals."—*From "The Change in the Climate," the book referred to in the extract from Sir Arthur Keith above.)*

"While the Drayson theory has not yet been generally accepted, and seems not to be generally known, it appears to be consistent with a large number of glacial evidences that have not heretofore been satisfactorily explained, and this theory or something like it bids fair to become recognised and established."—*The Glacial Theory and Drayson's Hypothesis, by Col. J. Millis, "Popular Astronomy,"* vol. xxii, No. 10, *U.S.A.*

"It is a fact that astronomers cannot explain the decrease of the obliquity, nor can they any longer pretend to know. Drayson not only explains this decrease and other enigmas, but his discovery is a means of introducing a great simplification in astronomical working. The explanation of the Ice Age in all its outstanding features is incidentally only one of the corroborations of this movement, of which there are many. Finally, if Drayson's course of the pole be not the true one, then the time has come for the oracular secret of the true curve to be revealed."—*Anonymous writer.*

[The recent pamphlet by Major Marriott, referred to on p. xix, has for its full title "Warmer Winters and the Earth's Tilt: the Ice Age fully explained": it is desirable to point this out, as it is known both as 'Warmer Winters' and as 'The Ice Age Explained'. It is obtainable direct from Messrs. Pollard, Bampfylde Street, Exeter, post free for 1*s.* 2*d.*; see last page of cover.]

SHORT SYNOPSIS OF PAPER.

Dr. Fountain's Paper consists substantially of eight long paragraphs, and the Reply quotes these paragraphs one by one and deals with the objections contained in them. But first attention is drawn to the fact that a correct knowledge of astronomical movements must precede any explanation of their cause, just as in a court of law the facts of the case must first be proved before the question of cause or motive can be entered upon. And it is asserted that orthodox astronomy, while offering explanation of the cause of the precessional movement of the pole, has never actually defined the course it takes.

Paragraph I.—Brief description of Drayson's polar movement, substantially correct, except that an important fact is omitted. One remark showing that the matter of the proper motion of the stars has been misconceived, is dealt with later.

Paragraph II.—Brief description of climatic consequences, substantially correct.

Paragraph III.—Complaint that Drayson treats the ecliptic as a fixed plane not subject to variation. In reply, this important question is threshed out, and it is shown that Drayson's reasons for regarding the ecliptic as fixed have never been controverted nor even examined. References are given for those who wish to go further into the matter.

Paragraph IV.—Same subject continued. "Looping the loop" analogy of Marriott termed "totally untrue," and an illustration given professing to show this. In reply, it is proved that Laplace's theory—which underlies this objection—is self-convicted of unsoundness when examined by spherical trigonometry. Further, it is shown that the illustration professing to demonstrate the fallacy of Marriott's statement, is itself unsound.

Paragraphs V and VI.—Professes to explain how Drayson was "misled" into thinking the ecliptic a fixed plane, and to show also how Drayson's calculations of the obliquity were effected. In doing this latter he commits a grievous blunder, and as a result produces discordances which are attributed to the failure of Drayson's system. In the reply, the blunder is exposed, the calculation being later worked correctly.

Paragraph VII.—Remarks on geology purporting to show that Drayson's conclusions were inadequate and insufficiently supported by facts. In reply Major Marriott's letter is quoted to show that Drayson did not profess to be a geologist and had only certain current writers to go by, yet it is noteworthy that no fact in geology since discovered is out of harmony with his conclusions.

Paragraph VIII.—Complaint that no attempt has been made to show errors in the accepted theory. In reply, errors previously pointed out (not only in theory but in actual mathematical procedure) are cited. The point is made that we do not know the CAUSE *of the Daily Rotation, yet we are able to study its phenomena: and it is urged that the same course should be taken as regards the polar motion—its true path should first be established, and the cause why that path is followed may be a subject for investigation later.*

This finishes Dr. Fountain's paper and the reply. The remainder is devoted to a further development of certain matters only briefly touched on in the reply proper. There is so much important material in these sections that it is not very easy to epitomise them. The Second Rotation is briefly described. The meaning of the term Proper Motion is explained, and the bearing of the Second Rotation thereon indicated. Then some hints are given to those who are sufficiently interested to make independent investigation, together with a re-working of Dr. Fountain's example. The unrecognised movement of the zenith resulting from the Second Rotation, and Sir John Herschel's implicit recognition of the latter, are mentioned. A suggestion is made bearing on Drayson's alleged change of opinion. A summary, with some concluding remarks, brings the tract to a close.*

The Appendix contains the two letters from "Science Progress" referred to in the body of the work (the reasoning in which is of some importance to the argument), with a chronology of the S.P. correspondence, a short bibliography, a quotation from Whewell's "Inductive Sciences," Astronomical Proof of a Recent Ice Age, etc.

* The bearing on this and preceding sentences of an original and novel investigation by the writer is indicated.

Rules of Reasoning in Philosophy.

Rule I.—We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

To this purpose the philosophers say that Nature does nothing in vain, and more is in vain when less will serve ; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

(*From Newton's "Principia," Book iii, Motte's Translation.*)

A REPLY

TO

"THE DRAYSONIAN FALLACY"

by E. O. Fountain, M.D., F.R.A.S.

(*Journal of the British Astronomical Association*, vol. xxxii, p. 107.)

The author of this paper makes at its close a remark of very great significance when he says that "Draysonians are in fact kinematicists, not dynamicists." Which being interpreted means that we concern ourselves chiefly with the study of motions, rather than with theories as to the causes which may produce those motions. And so we do.

This, however, is quite in order. Dr. Whewell in his *History of the Inductive Sciences* draws attention to the distinction to be observed between formal and physical astronomy. "In the introduction of the Copernican system into general acceptation, two leading views operated upon men's minds, the consideration of the system as exhibiting the apparent motions of the universe, and the consideration of this system with reference to its causes,—the formal and the physical aspects of the theory,—the relations of Space and Time, and the relations of Force and Matter. These two divisions of the subject were at first not clearly separated; the second was long mixed, in a manner very dim and obscure, with the first, without appearing as a distinct subject of attention; but at last it was extricated and treated in a manner suitable to its nature. The views of Copernicus rested mainly on the formal condition of the universe, the relations of space and time; . . . . The distinction of *formal* and *physical* astronomy thus becomes necessary, in order to treat clearly of the discussions which the propounding of the Copernican theory occasioned: (Third Ed., i, 272)." That is to say, it was first necessary to get a clear conception of the actual movements involved in the new view of our system, and definite evidence that such movements were those which actually occurred, before it was possible that any correct theory could be formulated to account for those movements on mechanical principles.—Copernicus, and the heliocentric doctrine, before Kepler: Kepler and the proved elliptic orbit of Mars (as attested by Tycho's observations), before Newton and the gravitational hypothesis.

In the passage quoted we have an accurate statement of the present conditions in regard to Drayson's problem. Here, as in the earlier case, the departments of formal and physical astronomy have long been mixed, "in a manner very dim and obscure," whence it results that the nature of the problem has not been, and is not, properly apprehended. And until students of astronomy recognise the proper functions of "formal" and "physical" in relation to this problem the question will make no headway, but will continue to be a ghost which refuses to be laid.

Up to the present time Physical Astronomy, trespassing upon the

unstaked ground of Formal Astronomy, has—to use Drayson's caustic phrase—invented an explanation to account for a movement which does not occur.

This remark will seem so surprising, to those who have never yet had occasion to reflect on the matter, that a little exposition may not be out of place. It will be admitted that if, for example, in an examination paper a student of mechanics be given a problem requiring him to explain the path of a body by reference to two given forces x and y, before he can be expected to give the answer he must be informed, and correctly informed, what path the body has been observed to take. If he attempted to write his answer without enquiring, we should set him down as a fool. And if he assumed it to be a circle, say, round some convenient point, and then, *without making experiment to see if this was so,* proceeded to write his answer on the hypothesis that his assumption was correct, we should think him a candidate who deserved to fail. And yet this is exactly the position of Modern Astronomy in regard to the motion of the pole!

From the time of Copernicus until now, orthodox astronomy has explained the precessional movement of the equinox as being due to a movement of the pole of the equator in a circle of which the pole of the ecliptic is the centre. The explanation of that movement has been given, and acclaimed as a triumph of Newton's genius. Then one day, two hundred years later, when a cadet at Woolwich put a question to his instructor which he answered according to currently received views, and then another question, the answer to which contradicted the former—*then*, it seems, at last it occurred to someone to investigate and see if the pole really did move in the manner ascribed to it (and "explained" on dynamical principles); and, as a result of his investigations, he found that it did not. He published his results, and the fun began.

It is likely to continue. Nothing can be more surprising to astronomers of orthodox training than the persistency of the Draysonian heresy. It is a curious fact that a discovery which resulted from a purely astronomical investigation, and which had as its concomitant a geological consequence rejected peremptorily by the geological leaders of its day, should at the present time find its most prominent supporters among the votaries of that very science, which now discovers (as does also archæology) that the *then* disputed dates are so much more in accord with the accumulated results of later research as to recommend the idea to them in the very face of a fully maintained astronomical veto. It is a curious fact. And it is not only a curious fact. It is a very significant one.

It may be objected that the foregoing does not deal directly with Dr. Fountain's statements, and that it is requisite that his summary of the reasons astronomers have for regarding Drayson's discovery as a "fallacy" should be replied to point by point. Indeed, in view of the fact that this is the *first time* that any such summary has been given, it might be said to be imperative, lest anyone should suppose that no answer is to be found.

The reasons for confining the above Reply to a general view were two. Firstly, the impossibility of dealing adequately with each point in the space

likely to be available to a correspondent in a controversy; and secondly—though by no means secondarily—the fact that the consideration there presented is *paramount, and must precede any discussion that is not to be an empty exchange of words.*

As regards the first reason, the case is now altered, since one is not here restricted to a page or two, while as for the second it is hoped that the reader will have fully digested the point by now. A detailed reply can therefore be given.

The Paper, as printed in the *Journal*, consists of twelve paragraphs, which become reduced to eight when the shorter ones are included in the larger to which they evidently belong. In order to prevent mistake, or trouble in referring, each of these eight paragraphs is quoted before the reply.

PARAGRAPH ONE.

> During the last few years there have been several references to the Draysonian theory in current literature, and suggestions have been made that this theory is capable of accounting for the Glacial Epoch. Drayson believed that changes in the obliquity of the ecliptic and in the positions of the fixed stars could be explained by the assumption that the pole of the heavens describes at a uniform rate a circle round a point 6° distant from the pole of the ecliptic in 31756 years, and that this point is so situated that the obliquity of the ecliptic will be at a minimum in the year A.D. 2295: [*footnote, saying* More strictly 2294.75]. At first he maintained that the fixed stars had no proper motions at all. He claimed by his data to be able to 'compute the Right Ascension and Declination of any star for any year in the past or future from one observation of that star.' This he clearly could not do, if there were any appreciable proper motions. Later on, however, he was obliged to give up this means of proving his theory, and 'recognised that many stars have proper motions of their own.'

Little exception can be taken to anything in this paragraph until we come to the last two sentences, and as a broad statement of Drayson's claims it is correct. Correct, that is, so far as any statement can be which omits a fundamental fact—a fact however which has no immediate bearing on the main subject of the paragraph, namely the Glacial Epoch. What this omitted fact is, and what its bearing is on the secondary subject of the paragraph, namely Proper Motion, will be indicated in due course.

The penultimate sentence shows an entire misconception of the point at issue, and demands treatment at some length. But as it is sometimes convenient first to enumerate points requiring elucidation or statements to be traversed, and enter upon their discussion later, we will content ourselves here with noting the fact.

In the last sentence ' Many stars have ' should read ' it may be that some stars have,' if it is to represent truly Drayson's own opinion as conveyed ir his letters to Admiral Sir A. de Horsey up to the time of his death.

PARAGRAPH TWO.

> If the axis of the earth rotates round a point 6° removed from the pole of the ecliptic, it is clear that its inclination has a range of about 12°. Assuming that the inclination will be least in the year A.D. 2295 and that the whole period is 31756 years, the inclination must have been greatest about B.C. 13584, and the Arctic circle would then come as far south in this country as Durham. In the

winter there would be much less sunshine, but in the summer there would be more, and the total amount of heat received from the sun at any place would be exactly the same as now. But the snow that fell during the winter months would not melt and run away, and the first part of the heat of summer would be used up in melting it, so that less heat would be available for warming the air and ground. Every year the snow would lie longer, especially on higher ground, till at length it would never melt, and permanent glaciers would form and gradually push forward into the valleys. On the other hand, at the present time, when the inclination is getting less, and is not quite at the minimum, the difference between summer and winter should be getting less marked ; and in this way can be explained why latterly we have had such mild winters, and also why the summers have been so short and cold.

This paragraph does not call for comment. It is an admirably concise description of the main features of the climatic cycle.

PARAGRAPH THREE.

There is no great difficulty in finding one of the sources of Drayson's original error. I have said that he makes the pole of the earth describe a circle round a point 6° from the pole of the ecliptic, and denies any further motion. In other words he treats the ecliptic as if it were an invariable plane. This is quite clear. On page 197 of his " Last Glacial Epoch " he says : ' After many months of careful research we found that there was no evidence worthy of the name that could have been made to prove from the changes in stellar latitudes that the plane of the ecliptic has varied during the last 1700 years.'

This is a very important paragraph. Very important indeed. One of the sources of Drayson's original error, we are told, was that he treats the ecliptic as if it were an invariable plane ; and a sentence is quoted showing that Drayson had come to this conclusion after many months of careful research. This sentence, if looked up, will be found to follow an account of the means he adopted to settle the question as to whether the ecliptic had moved perceptibly during historic time, and the reasoning employed. In the light of this investigation he considered it had not. Is it not a little curious, then, that the false reasoning by which Drayson came to this erroneous conclusion should not be cited ? And further, that the superior and correct reasoning by which official astronomy has concluded from the observations available that the plane of the ecliptic *had* altered during historic time, is not laid alongside Drayson's so that his fallacious processes should be thrown into strong relief ?

The present writer can only repair one moiety of this omission. He can but give Drayson's procedure, taken from the page referred to. ' We took,' says he, ' at random four or five stars, having a longitude nearly the same and about 90 degrees [*i.e.*, near ♋0°], and we compared the changes in latitude shown by these stars between the dates of Ptolemy's and Ulugh Beigh's catalogues [140 A.D.—1437 A.D.]. Between the catalogues of Ulugh Beigh and Hevelius [1437—1660], and by converting declinations obtained by latest observation into latitudes, we obtained another comparison. We then examined the changes in latitudes thus obtained with the changes exhibited by a similar comparison on stars differing 180 degrees in longitude from those previously examined [*i.e.*, near ♑0°]. This process was then adopted on stars differing about five degrees in longitude from the former series, so that a very extensive test was obtained. We next selected nearly all the stars of Ursa Minor, Draco, Cepheus, and others near the pole of the

ecliptic, and endeavoured to trace by changes in latitudes what had been the movement of the pole, and hence the plane, of the ecliptic. After many months' careful research we found that there was no evidence worthy of the name that could be made to prove from the change in stellar latitudes that the plane of the ecliptic has varied during the last 1700 years.'

Well, one may ask, is not that a scientific way of testing the matter? He tells us fairly and fully on what evidence he based his conclusion. And to controvert it, there must either be produced countervailing evidence, or, we must be shown that such has been produced, and where it can be examined. This countervailing evidence the author has failed to find in the numerous treatises he has consulted, ranging from Vince's *Complete System of Astronomy* and Laplace's *Mécanique Celeste* up through Grant's *History* and two editions of the *Encyclopedia Britannica* to Newcomb's *Astronomical Constants* and the same author's *Spherical Astronomy*. But Vince is as explicit as anybody, and all he says is (Art. 1070): 'By comparing the modern with the ancient observations it appears that the latitudes of the fixed stars vary. . . . Kepler and Tycho observed that the latitude of the stars was subject to a change; the former concluded that it was owing to a change in the position of the ecliptic, and that it arose from some physical cause.'' No details are given, nor are references made to the original observations or to authorities where they may be found.

On the face of it, then, it would seem that Drayson is right. He investigated the matter, re-examined the evidence, and in the light of that examination gave his verdict as *not proven*; 'other evidence,' he said, 'of this variation of the ecliptic there [conceivably] may be, but it is not from changes in latitudes of stars.'

'Oh, but we know——' the reader may say, 'we know from the theory of universal gravitation that there *must* be changes in the obliquity of the ecliptic.'

In saying this he is in good company. For all the 'authorities' exhibit false reasoning of the same character. And it affords an apposite instance of that 'mixing, in a manner very dim and obscure,' of the parts of Physical and Formal Astronomy already alluded to in preceding remarks. We do *not* KNOW, from the theory of universal gravitation, that changes occur in the position of the ecliptic. We *infer*, from the generally satisfactory accordance of many facts revealed by observation with the explanation of those facts afforded by the gravitational hypothesis, which induces us to conclude that it is a true explanation,—we *infer* that certain corollarial phenomena logically deducible from this theory will be found to occur; and one of these would be a change in the position of the ecliptic with reference to the stars as a whole. But that is a very different thing from KNOWING that such phenomena actually happen. And until it is established by observation that they *do*, it is not legitimate to treat them as facts, nor to use them as foundations for further theories.

To conclude on this point. We have seen that Drayson gives his reasons for declaring that the ecliptic is to all intents and purposes a sensibly invariable plane. These reasons have not been shown to be fallacious, nor the evidence on which they are based unsound. And until that is done, we may reasonably

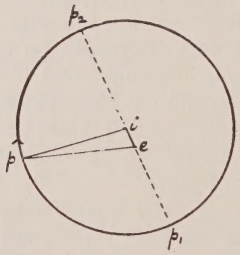
presume that he is right. Those who wish to go deeper, and follow the progress of his later researches in this matter, may read and compare *Glacial Epoch* 197, *Fixed Stars* 96, *Untrodden Ground* 232, *Earth's Past History* 63, and context of each.

PARAGRAPH FOUR.

> It is this assumption that the ecliptic is an invariable plane that seems to be continually leading Draysonians into erroneous statements. For instance, a Draysonian states: "As a matter of fact the distance between the pole of the heavens and the pole of the ecliptic, which is the measure of the obliquity, can no more be affected by the changing inclination of the earth's orbit to the invariable plane, than looping the loop can affect the distance separating the pilot and the observer." This is totally untrue. Obviously if there were no precession the axis of the earth would always point in the same direction in space, it would maintain a constant angle to the invariable plane, and the obliquity would vary exactly with the 'changing inclination of the earth's orbit to the invariable plane.'

The close juxtaposition here of the phrases 'the ecliptic is *an* invariable plane' and 'inclination of the earth's orbit to *the* invariable plane,' may tend to fog the reader. But by an apparent digression—a digression very material to the issue though, as will be seen—we may clear away some of the fog.

Fig 1



First, what is the Invariable Plane? Laplace in his *Mécanique Celeste* showed that from mechanical considerations according to the theory of universal gravitation, there should exist in the solar system an 'invariable' plane, which may broadly be said to be the mean of the various planes of the different planetary orbits. He stated that the position of this plane could be found, mathematically, by the condition that the sum of the products of the mass of each planet by the area swept out on this plane by the radius vector, should be a maximum. He gave the positions of this plane separately calculated for 1750 and 1950 and sensibly identical: (Livre ii 62, vi 46).

Here, then, we have an invariable plane duly accordant with gravitational and mechanical theories or principles. These theories further require, as indeed is implied in Paragraph Four, that the mean angular distance of the terrestrial pole, and the pole of the Invariable Plane, does not change: (*Encyclopedia Britannica*, Ninth Edition, ii 794). Now the Invariable Plane makes an angle with the plane of the ecliptic of about one-and-a-half degrees, and its ascending node is about longitude 106° on the celestial sphere: its pole is in R.A. 273° ·9, N. Dec. 66° ·9, while the pole of the ecliptic is in R.A. 270°, N. Dec. 66° ·5. The relative position of these three poles, therefore, is shown in the annexed figure (Fig. 1), where *i* represents the pole of the Invariable Plane, *e* the pole of the Ecliptic, and *p* the terrestrial pole or Earth's Axis—or, to be more precise, the point of the celestial sphere which this axis would cut if indefinitely prolonged.

Theory requires, as we have seen, that the side *ip* should be constant, whilst for short periods *ei* may also be regarded as constant.

By the motion of precession *p* is carried towards *p2* by a clockwise motion indicated by the arrow. It is evident that as *p* moves towards *p2* the side *ep* must increase, since in the position *p2* its length is *ip* plus *ei*, whilst in the position *p1* its length is *ip* minus *ei*.

The lengthening of *ep* however implies that the obliquity is *increasing*, whereas from observation in our own times we know it is *decreasing*. So that the only way to make theory and observation agree would be to take the motion of *p* as being towards *p1*, instead of towards *p2* ; that is, to suppose that precession takes place in the direction of the signs ♓ ♈ ♉ instead of in the contrary direction ♉ ♈ ♓ which observation assures us it does. In other words, to make things fit, a plus sign needs to be used in the equations where a minus sign ought really to be employed ; *and that is what Laplace has done !*—presumably through an oversight : (*Méc. Cel.* livre VI, No. 31).

The mistake was not discovered until over a century later, and when it was, the discoverer was not thanked. His letter on the subject to *Nature* was not inserted, and his pamphlet was generally ignored.* There were not wanting, of course, amiable critics who had no great difficulty in assuring him he was wrong, and in explaining the meaning of the equations in the *Mécanique Celeste* which he had so evidently misunderstood. But—and the significance of the fact will be apparent—they none of them so much as mentioned the GEOMETRICAL demonstration which he had given ! And in a reply to his critics he naturally twitted them with the fact.

" But what bearing has all this on Paragraph Four ? " the reader may ask.

A very important one. It shows that mechanical and gravitational theories, even when applied by their ablest mathematical exponent, are unable to account for the observed decrease in the obliquity. Further, the exposure of an error so glaring in its nature and so important in its consequences, which remained *undetected for nearly four generations* and is *still unacknowledged*, disposes of the assumption too frequently made that there can be nothing seriously wrong in the fundamentals of astronomy, or ' surely it would have been discovered ere this and admitted and rectified as soon as pointed out.'

And it has a further bearing. For it was in consequence of this investigation that Laplace (and later Stockwell, who repeated the calculation with new values of the planetary masses but without discovering his predecessor's mistake)—it was in consequence of *this* investigation that Laplace was led to declare that the limits of the variation in the obliquity were about $1\frac{1}{2}°$ on either side of a mean ; *which statement, half a century later, proved to be the main obstacle in the way of any unbiassed examination of Drayson's*

* *A Gigantic Hoax*, by G. E. Sutcliffe, 1906 : (rare ; may be seen at the British Museum Library), from which the above account is taken. As this charge is so serious, it may be proper to remark that the writer was not content to take it on trust, but that he was at pains to verify the references and examine the matter for himself ; and that, moreover, he followed very attentively the correspondence which ensued in the only two magazines which opened their columns to a discussion on the subject.

claims. For, it having been so long considered established by a specific investigation that 3° was the extreme limit of change, men's minds had become fixed and set in that belief as strongly as though it were a known fact!

Stockwell's re-determination* being confined to this particular item of the 'limit of variation' in *ep* when *p* moves through a complete cycle of 360° or plus-minus 180°, explains, and perhaps excuses, the fact that he failed to detect Laplace's blunder; for the discoverer says he only found it out through investigating the matter geometrically. And this occurrencee within a year or two of Drayson's death supports his view that the neglect of geometrical, in favour of the almost exclusive employment of algebraical or 'analytical' methods, is both a serious danger to science and mischievous in its effects on education generally. (See Note in Appendix on this head.)

But it is worth pointing out as an interesting fact that what Laplace did was virtually to assume exactly the same type of movement as Drayson is represented as having done, placing however *his* centre $1\frac{1}{2}$ degrees from the pole of the ecliptic instead of 6 degrees, and in an altogether different direction.

In view of the state of things here revealed, it may be considered that the criticism contained in this Paragraph Four requires no comment. We will however examine it.

What exactly is it that is here declared to be "totally untrue"? It requires a little thinking out.

The direction of the Earth's axis is relatively permanent. That we know; for it does not sensibly change from year to year. It is not absolutely permanent. That we also know; for the phenomena of precession, and of the decrease in the obliquity of the ecliptic, show (*a*) that its direction as regards the present position of the solstitial colure changes, and (*b*) that its inclination to the ecliptic changes. These two simultaneous movements being put together, indicate that the earth's axis describes a cone (double-cone), or some figure nearly resembling that. The question then arises, what is the nature of this figure? Is it a (double) cone, or is it only conoidal? And if a cone, what is the axis of that cone? It cannot be the pole of the ecliptic, as was originally supposed, since the distance between *p* and *e* is not constant. Nor can the centre or axis of this movement be the pole of the Invariable Plane for the reason we have just seen. And yet only if it were could the last two sentences of Paragraph Four apply!

And *even then*, they could apply ONLY on the condition that the period of precession was exactly synchronous with the period of oscillation of the plane of the ecliptic, a state of things which has never been claimed to exist. In order to follow out clearly the movements described in the Paragraph it is necessary to take some tangible object, say a table, to represent the Invariable Plane, a sheet of millboard to represent the oscillating Ecliptic plane, and a ruler to represent the Pole of the Heavens—first suppressing its precessional movement, and afterwards restoring it, as suggested. The truth of the above remark will then become apparent, and it will be seen

* *Smithsonian Contributions to Knowledge*, vol. xviii.

that even on the basis of his own contention Dr. Fountain's criticism stultifies itself. But it will be noticed that this Paragraph Four implicitly assumes that the pole of the ecliptic is *not* the centre of the polar movement, but that instead the pole of the invariable plane is the centre.

As to whether the motion is conical or only conoidal—or, to put it differently, whether the path of the pole p is a circle or some other curve,—that is the very question which Drayson, who was the first man to investigate the matter, set himself to solve. He gave an answer, and a very definite one, which has never yet been examined by astronomers with the attention which it merits. Certain consequences ensue from the answer which he gave and some of these will occupy our attention before closing.

PARAGRAPH FIVE.

It is easy to see how Drayson was misled into looking upon the ecliptic as an invariable plane. In the mechanical theory of precession it is necessary for the sake of simplicity to do so temporarily. As the movement of the ecliptic is small the main part of precession about the temporary position of the pole of the ecliptic would not be appreciably altered for an infinitesimal time, and to find the obliquity of the ecliptic this would have to be combined with the ecliptic's own movement. This, of course, follows a complicated formula of many periodic terms. Therefore the trace of the pole of the earth on the celestial sphere would be a very complicated curve. For a short period, however, it would not differ very much from a circle round the pole of the ecliptic. For a period a little longer a circle of a different size and round another point can be made to fit better, for we have now three parameters at our choice, one for the radius of the new circle and two for the position of its centre. It seems a fairly simple process to discover these, and it is difficult to understand why Drayson spent "many years of labour" in the process, unless, since he was concerned not only with the problem of accounting for the changes in obliquity of the ecliptic but also with getting rid of the proper motions of all fixed stars, he had to keep on changing his three parameters backwards and forwards with a view to the latter, and could not keep to the very simple problem entailed by the former. Let C be the centre of Drayson's circle, E the pole of the ecliptic, P the pole of the earth's axis at any time. Then according to Drayson, P moves uniformly, and therefore the angle ZEP [see figure given overleaf]

$$= \frac{\text{Date at which } P \text{ will reach } Z - \text{present date}}{\text{No. of years } P \text{ takes to describe circle.}} \times 360°.$$

PE is the known obliquity of the ecliptic at the date.

$$\cos PC = \cos PE \cos EC \text{ minus } \sin PE \sin EC \cos ZEP.$$

By taking, therefore, four positions of P at four different dates and equating them, we can by a series of approximations easily find the three parameters.

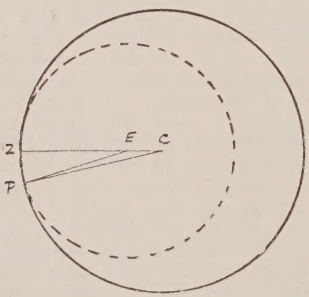
The somewhat lofty tone of the earlier portion of this paragraph "cuts no ice" as the Americans say, in view of the necessary distinction between *formal* and *physical* already mentioned and the reasons found for regarding the ecliptic as being, so far as observation affords evidence, sensibly invariable.

The remainder of the paragraph is, in effect, devoted to showing how wofully ignorant Drayson must have been not to know that it is always possible to choose a circle such that its circumference will touch and for a short distance sensibly coincide with any given curve. And yet Drayson could hardly have been ignorant of such mathematical devices as the 'circle of curvature,' etc. Indeed, we know he was not. The properties of the circle of curvature are dealt with in every elementary treatise on the differential

calculus, and it so happens that in his " Experiences " he mentions that his preparatory course for Woolwich—in which, by the way, he came out first in the final examination—in mathematics included trigonometry, conic sections, statics and dynamics, maxima and minima, projectiles and *the differential calculus* (*op. cit.* p. 211).

But every critic since Drayson first wrote has taken this line. The implication thus subtly conveyed is that Drayson had found a mare's nest. Had he only been acquainted with the properties of the ' circle of curvature (so runs the implied argument), he would have known, poor fellow, that his circle of polar motion was merely the circle of curvature of the true curve, and would not have been so elated at a supposed discovery. The weak point in this line of attack is, that since the ' true curve ' has *never been defined*—notice how our present critic avoids doing so !—there is no real ground for concluding that the ' circle of curvature ' cannot be itself the true curve.

Fig 2



It would have been more to the point if any instance had been given showing that Drayson did in fact display such ignorance. His critics seem in general remarkably shy of quoting from his books ; and nowhere do they mention the fact that he has cited and replied to every one of these objections in his published works. Still less do they instance any false reasoning in these replies. One might ask why. Is it, perhaps, that the patronising tone which they manage so well would become more difficult to maintain when placed in confrontation with Drayson's own language ?

We now come to something more serious, for at the close of the paragraph an unaccountable misrepresentation of Drayson's system is given. It is so amazing a one, that at first it seems as though a misprint must be the explanation. But the equation shows that the statement, misleading as it is, is intentional—though of course one does not mean to imply that it is intentionally misleading. Had the author been a little less confident of Drayson's folly, he would no doubt have taken pains to read those portions of his works which he has chosen to consult, with more attention.

After giving the diagram which now follows,* he says :—' Then according to Drayson *P* moves uniformly, *and* THEREFORE the angle *ZEP* etc.'

* Fig 2. [The broken circle, which shows the polar path as generally received, has been added.—A.H.B.]

But what Drayson said was, not that *P* moves uniformly round *E* as a centre, but around *C*; whence it follows that the equation gives the value of the angle *ZCP*, and not of the angle *ZEP* as it is stated to do, *and as Dr. Fountain in the next paragraph uses it to do.*

PARAGRAPH SIX.

The question now arises as to how far Drayson's contention that his movement of the pole will give correctly the obliquity of the ecliptic for all time [is true, or will stand examination].* It is obvious that for a short period, especially at times near to those at which he has chosen his parameters to fit the theory, the differences must be very small. To avoid any question as to the accuracy of the figures employed in the calculation we will take those for the obliquity given by Drayson himself. Let us compare the values of *CP*, obtained from the value of O.E. for 1890, that of Bradley in 1755, and of Ptolemy in A.D. 140. The angle *PEZ* and *CP* are calculated.

Year.	*O.E.*	*Angle PEZ*	*CP*
1890	23° 27′ 12″·79	4° 35′·3052	29° 26′·29
1755	23° 28′ 17″·6	6° 7′·1304	29° 26′·64
140	23° 50′	24° 25′·6320	29° 23′·70

On comparing the years 1755 and 1890 we see that while the O.E. has changed by about one minute, Drayson's radius, which should have remained unaltered, appears to differ for the two years by a third of a minute. He has not, therefore, been even so successful as might have been expected in making things fit. As regards his radius in A.D. 140 it might be contended that Ptolemy's value of the O.E. is only supposed to be correct to the nearest 10′. If it had been 3′ more, the value of Drayson's radius would have been 29° 26′·69, and therefore near its value for 1755. However, it follows that although by a proper choice of parameters Drayson's theory can, of course, be made to fit for a few epochs, it cannot be made to do so for all.

This paragraph, since it professes to deal in a quantitative manner with Drayson's problem—to disprove it by showing its non-accordance, not with dynamical theories but with the facts of observation—has an importance different in kind from its predecessors and therefore calls for special consideration.

Its object is to show that when we take Drayson at his word, we find that we require a different radius for *CP* at different dates. But how does our sagacious critic go about his task? Calculating the angle *ZCP* by the formula given above, he *calls it ZEP*. With this angle (that is to say with its supplement) as *PEC*, and with the two sides *PE* and *CE* he solves the triangle *CEP*. He performs this operation thrice, and then triumphantly points to the fact that he obtains different values for *CP* each time!!! Three notes of admiration cannot be too many here.

Fortunately, though less excusable perhaps than that of Laplace this amazing blunder will not have such disastrous consequences. The former delayed the onward march of science a whole century, while this only——.

'Oh, but wait,' the reader may interrupt, "you said just now you thought at first it must be a misprint. How can we be sure that it isn't the printer's mistake after all?'

Quite simply. Just work the figures out yourself. The year when *P*

* Sentence is not concluded in the original.

will be at *Z* is given in Paragraph One as 2295 A.D. and the length of the cycle as 31756 years. Accordingly the angle *ZCP* is—

$$(360^\circ \div 31756) \times (2295 - 1890) = 40''{\cdot}8 \times 405 = 4^\circ\ 35'{\cdot}4$$

which, as we see, is given in the column as the angle *ZEP*. Then our quantities for the triangle *CEP* are (180° − 4° 35′·4), 6°, and 23° 27′·2; whence we find for *CP* 29° 26′·3 as shown above. And the others are worked in the same way.

A more intelligent way of attempting the criterion here aimed at, would have been to take the angle *PEZ* as decreasing uniformly at its present rate which is the precessional rate of $50\frac{1}{4}$ per annum. This would furnish a rough test of the accordance of Drayson's radius with known obliquities, although it is manifest from the diagram that *PEZ* cannot decrease quite *uniformly*,* at any time, and hence the results at best will only be approximate as we shall see later. In the work as it stands, its author has perpetrated a veritable "howler." Is it in such slipshod fashion that heresies are to be refuted or fallacies exploded ?

PARAGRAPH SEVEN.

> It is obvious that some Draysonians have not adopted Drayson's hypothesis from primary considerations, but because they find that it harmonises with their conviction that the glacial epoch was synchronous round the North and South Poles, and this does not agree with Croll's theory. However, if Croll is wrong, it does not follow that Drayson is right, but only that the glaciation was due to a cause acting upon the whole Earth at one time. Under any circumstances a difficulty would be that, under Drayson, the temperate regions of the Earth should be glaciated for half its life, reaching a maximum every 32,000 years. Drayson tries to show that hot and cold periods have always alternated. He makes the colder cycles the Cambrian, Devonian, Permian, Cretaceous and Glacial Periods, while the hotter were the Silurian, Carboniferous, Jurassic, Tertiary, and present day. It is obvious, however, that these could not have been all of the same duration, and that 32,000 years is insufficient for any pair. In another place he appears to consider that the 32,000 years represents only one of the cold sections of the whole glacial epoch, with its preceding or following genial period. If so, where are the traces of the numerous glacial periods which should, in the absence of exceptional circumstances, have occurred many times during each of the previous long hot periods ? The change of climate that should have occurred in the last two thousand years is not proved. Wine has been made of late from grapes grown in England, and the range of peaches and figs seems about the same as in the monastic gardens in the Middle Ages.

'He makes the colder cycles the Cambrian. . . .' This reference to the succession of geological periods Cambrian, Devonian, etc., appears to be taken from page 49 of the *Glacial Epoch*, from a chapter consisting of extracts from geological works—in this case from Page's *Past and Present Life of the Globe*. So that the statement is not Drayson's at all.

However, this Paragraph deals with geology, and as our concern is with astronomy it might be passed ; but Major Marriott's remarks on it, in a letter to the present writer are worth quoting :—' It seems to indicate a 'wrong conception of the attitude of Drayson towards geology, of which

* The precessional rate, now 50″·25 was taken by Hipparchus as 48″ (*Mem. R.A.S.*, xiii, p. 15, *note*), which is just what it should have been according to Drayson (*Draysonia*, p. 35). This point will be returned to later.

'he had no knowledge beyond the current opinion that some geologists 'held at the time, and whom he quotes in Chapter II of his *Glacial Epoch.* 'He was content to satisfy himself that geology was in the main not inimical 'to his discovery ; and it should be noted that while in geology there has not 'been a single fact brought forward of late years which is at variance with 'the deductions made by Drayson, facts corroborative of his theory from 'geological and extraneous sources have increased in number and importance.

'"The change of climate that should have occurred in the last 2,000 'years is not proved." That may be a matter of controversy when omitting 'the recent decades, and it will be seen to be in accordance with the diagram 'of the obliquity curve furnished by de Horsey and printed in *Warmer 'Winters*, but at the same time it is universally recognised in Northern 'Europe, as also in the Falkland Islands, that the climate *has* changed, 'which is indicated by the marked recession of glaciers and the retreat of 'the polar fringes. Nor is a reasonable explanation wanting, though it has 'not been given in this form before. We are dealing with a substance anoma- 'lous in its physical properties. Ice at 32° F., before becoming water at '32°, must absorb about 80 units of heat, though one unit of heat at other 'temperatures is sufficient to raise it one degree. It is therefore not difficult 'to realise that during this long era the slow rise in temperature of the ice 'throughout its depths was obscured, and that a large belt of ice-retaining 'areas may have been absorbing units of heat until the whole mass approached 'in temperature nearer the critical point, so that a stage* has been reached 'some seventy years ago, which has increased the discharge of bergs and 'floes into the temperate zones, and sensibly decreased the icebound areas 'of the polar confines, raising the temperature of winds originating over 'them. Thus there has been rather suddenly revealed to mankind the fact 'that a change has been slowly taking place, though it has eluded observation 'until now. The sighting of an iceberg off the Sandwich Islands this year 'for the first time in history is a very significant fact. Of similar importance 'was the notice from Newfoundland at the beginning of March that the ice 'floes coming south were earlier and of greater extent than usual, while the '"Quest," previously in the Antipodial spring, had reported that the ice- 'packs were floating much further north and in greater quantities than 'previously experienced. The past winter has been comparatively mild, 'while the summer has been an unusually cold one.

'Nor are we unprovided with an illustration of the process, above des- 'cribed, nearer home. Last year two Alpine glaciers, at Grindelwald and 'Chamounix, suddenly slid with destructive effect far beyond their normal 'limits. This shows that the internal temperature of the ice mass had risen 'to a point at which it was not low enough to cause immediate re-freezing 'on the removal of pressure ; for it must be remembered that the plasticity 'and slow flow of glacier ice is due to the fact that when subjected to a certain 'pressure of resistance—as when its under-surface has to rise over a rock— 'the contiguous ice melts, but freezes again directly the pressure is removed.

* We know that the retreat of the last ice invasion is marked by equally prolonged stages of pause in retrogression.

'But in this case, of higher general temperature and absence of re-gelation 'process, the glaciers acted in the manner of snow sliding off a roof.

'Now, the Polar seas are fed with icebergs by the enormous glaciers 'which are the feature of Greenland; and one can readily see how by analo- 'gous conditions of increased internal temperature, the launching of these 'bergs may be accelerated, and their yearly output from time to time 'increased.

'An item that is not quite beside the point in this connection is the 'evidence of the "Submerged Forests." The late Clement Reid stated 'in his evidence before the Committee on Coast Erosion in 1911—"About '4,000 years ago there set in a fairly rapid but intermittent subsidence 'of the land, or rise of the sea. *We don't know which it was, but for practical 'purposes it is the same.*" (Evidence, vol. iii, Part I). This admission is 'important, because many reading his book, in which the phenomenon 'is referred to as a subsidence of the land, seem quite to overlook the converse 'and more reasonable probability of a great accession to the volume of the 'Ocean by the gradual (or rapid) melting of vast areas of continental ice- 'sheets. Though many computers from time to time write of a rise of 'hundreds of feet, even tens of feet would suffice for our evidence.'

PARAGRAPH EIGHT.

Obviously an hypothesis that the recognised movement of precession is incorrect ought to show some errors in the accepted mathematical theory. No real attempt appears to be made to do this. We meet only with vagueness. Speaking of Newton, a Draysonian says:—"Judging the earth to be symmetrical in shape, this was the best solution he could give, though it was scarcely in accord with one of the corollaries of his Laws of Motion, which demonstrates that one body may be considered to act upon another body at its centre of gravity, irrespective of figure. Newton, in his *Principia*, states, regarding the above corollary, that, with an oblate spheroid, this is not exactly true. 'Why, exactly ?' one may ask."* Certainly one may ask it. and will then discover that Newton was right. The notion that the resultant attraction on a body may be considered always to act at its centre of mass is the point upon which another astronomical heretic contrived to impale himself. Other suggestions in this direction consist only of references to 'disparity in density and distribution of continent and ocean.' Now in the theory of precession the only way in which these are involved is in the three moments of inertia. If these be called A, B, and C, C being the moment round the polar axis, it is generally assumed that A and B are equal. If they are not, then for complete stability it would be necessary that—

$$\frac{A\ (A-C)}{B\ (B-C)}$$

should not be very different from unity. This requires that the difference between A and B should be small compared with the differences between C and both of them. But the rotation period of the earth is a long distance from that at which an ellipsoidal arrangement is stable for a fluid body, and, even if it is solid throughout, an ellipsoidal arrangement would produce stresses that could not be long resisted in a body which is continually yielding to smaller ones with the production of earthquakes. Hence A and B must be nearly equal, and hints on these lines that the dynamical theory is wrong are powerless to shake it. But in truth Draysonians pay scant attention to dynamics; they are, in fact, kinematicists, not dynamicists. [End of the Article.]

* *Science Progress*, April, 1919, p. 596: (this footnote is accidentally misplaced in the original). The passage is correctly quoted, yet it seems as though a misprint must have occurred in the original and that "Why 'not exactly'?" was written].

' Obviously an hypothesis that the recognised movement of precession is incorrect ought to show some errors in the accepted mathematical theory.' Agreed. Yet now that we have drawn attention to errors, and glaring errors, in actual mathematical procedure shall we be approved ? Time will show.

But since this Paper occupies itself in the main with general arguments, rather than with particular considerations, it will be juster to deal with it on such issues—as indeed has been done throughout. Here we are once more brought up against that standing bugbear of the question, the Newtonian explanation of precession. Although complaint is made of the vagueness of Draysonians, this Paragraph itself is not so lucid as it might be. The question is as to why the effect of gravitational attraction on an oblate spheroid should ' not exactly ' be equivalent to action at the centre of gravity. The reason that concerns us is not simply that it is an oblate spheroid, as the remarks would seem to imply. The whole point is that it is a *rotating* spheroid, and that the direction of the attraction of the sun and moon on the equatorial protuberance is oblique to the plane of rotation. The differential effect of the pull on the ' near ' as compared with that on the ' off ' side, small relatively though it is, thus tends to exercise a slight ' downward ' pull on the equator in the direction of the ecliptic. And this downward pull combined with the tendency to fixity produced by rotation, causes the axis—not to bend down towards the ecliptic exactly, but to ' sway round from the hips ' as it were, or, in the usual expression, perform a conical movement ; and from this arises the retrograde movement or ' precession ' of the equinoxes.

That is the Newtonian explanation of precession according to the theory of gravitation, and it is so ingenious that one is hardly surprised that astronomers are inclined to believe it true, and to complain that we are upsetting the whole structure of Modern Astronomy if we object to it as being Not Proven. That is to say *not proven* by comparison of the path thus assigned to the pole, with that path as traceable by reference to recorded observations.

For it seems to be altogether overlooked how many tacit assumptions there are in that explanation. A mathematical demonstration is, after all, only a more elaborate form of syllogism ; it proceeds by a complicated yet purely formal logic from certain implicit premises to certain expressed conclusions. Here the implicit assumption is that the centre of gravity (or centre of mass if that term be preferred) is the centre of figure, or sensibly so ; and that the figure is symmetrical about its axis of rotation, or sensibly so. We do not know that these implied conditions are true, though it certainly seems fair to assume them provisionally as a starting point for hypothesis. But that does not justify us in assuming that they must be true and in refusing—tacitly rather than avowedly—to devise methods or examine evidence which will either establish or disprove the assumption. Newton's theory was propounded in the days when the obliquity was generally supposed invariable. His theory pointed to the pole of the ecliptic being the centre of the precessional movement. When it was established beyond question that a progressive decrease in the obliquity took place, astronomers should have realised at once that since this pole necessarily could not be the *centre* of that movement some other point perhaps might, and should have in-

stituted an enquiry to discover it. Official astronomy, however, omitted to do so, and it was left to a volunteer, Drayson, to be the first man to undertake this work in an intelligent and systematic manner. How intelligent and how complete his investigation was, astronomers generally have not at present realised, though in time no doubt they will.

But, it may be objected, surely geodetic measurements indicate just such a figure of the earth as Newton required, and geodesy and precession are said to afford mutual corroboration? True. They are said to. But there are those who assert that geodetic measurements do not bear out the claims made in their behalf; that, in fact, theory has modified the results explicitly or implicitly before ever they come to publication, and that at best results are scanty, discordant, and rarely if ever verified over the same ground where a very large arc of the meridian is in question: *e.g.*, J. von Gumpach, W. Leighton Jordan, and others. They, however, are in a small minority, and in view of the difficulty of getting a hearing for Drayson one cannot be surprised to find that their writings are hard to come by! Still, these views *are* maintained, and that should prevent us from making a foregone conclusion of the matter. Some propose a different figure, pear-shaped or heart-shaped, and recently Jeans has argued for a pear-shape from purely mathematical considerations. The settling of a question like this experimentally is not easy, since the equatorial 'protuberance' (if any) is so slight. The excess of the equatorial over the polar diameter is adjudged to be about 26 miles, or roughly one three-hundredth of the whole. If we imagine a faulty billiard ball three inches in diameter, and measuring a *hundredth of an inch* more one way than the other, and if we suppose we have to detect this unevenness not by direct measurement with calipers or a micrometer gauge, but by a comparison of the *measured lengths* of small arcs of 12° or less on the surface of the sphere, it will give us a picture of the problem set before geodesy.

One fact should not be lost sight of. It is slurred over in the text books. It is this. We are entirely ignorant of the cause, and of the nature of the maintaining power, of the daily rotation of the earth. In any ordinary study of the matter we treat the earth as a top, set going *somehow*, in the far past; and, moving as we suppose in a frictionless medium, it has not yet run down. But that is not really knowing the cause; it's just pretending! Yet we do know that whatever the cause may be, electro-magnetical or what not, this rotation is steadily maintained; and we are able to study its period, direction of axis, etc. Would it not be simpler, and in the long run more scientific to study in the same way the period, direction of axis, etc., of the Second Rotation of the Earth which Drayson showed was competent to explain the facts of precession and decrease in the obliquity, as well as certain data of geology not at the time discovered or at any rate not recognised; would it not be better to study the NATURE of this movement first, and reason as to its CAUSE later?

Each paragraph of Dr. Fountain's paper having now been separately

recited, and examined,* the Reply proper may be regarded as concluded and what remains as more or less supplemental.

It has been necessary to speak very plainly in showing the unsatisfying nature of these alleged proofs of the Draysonian 'fallacy,' yet it should be made clear that we have no special quarrel with the author who though he is no better is certainly no worse than others who have written in the same strain. His careless oversight in Paragraphs Five and Six is symptomatic of the hasty and superficial way in which he, in common with all others—as for example the late eminent editor of *Nature* in 1873 and 1874—has examined Drayson's work before sitting down to pass judgment on it. In his excuse it may be urged that it was after all a 'slip' and so no doubt it was, but, and this is the point—had he not been so filled with the complacent assurance that Drayson had found a mare's nest, he could not have conducted his examination so carelessly as *not to have discovered the slip, after having made it.* And the same attitude was exhibited in Paragraph Four, where it betrayed him into another hasty conclusion. These characteristics are not peculiar to himself but common to the class he represents. The capacity to make considerable attainments in those fields of knowledge where the path is well trodden by no means necessarily implies the ability to examine, freshly, old problems, or to divest the mind of preconceptions when approaching something new.

The author of this Paper has performed a service to astronomy in compiling it, and he has made it thoroughly representative of the views and methods of reasoning of the orthodox astronomy of our day.

The Second Rotation.

In commenting upon Paragraph One (p. 3) it was said that a fundamental fact was omitted in the description of Drayson's movement. This fundamental fact was the Second Rotation. It was a serious omission, though doubtless the author would not realise this, since he evidently does not understand the matter sufficiently to appreciate its importance.

Drayson's discovery in brief was this. In addition to the daily west-to-east rotation about an axis fixed in position in the earth itself, the earth has also a slow 32000 year east-to-west rotation about an axis fixed in position not as regards the earth but as regards the heavens. The position of this axis, its northern pole, is situated in Right Ascension 270° (about) and North Polar Distance 29° 25′ 47″. Quite a simple conception when once grasped, as indeed was Galileo's of the earth's daily rotation, though it took a long time to gain acceptance.

It may be asked what evidence there is of this Second Rotation. The answer is, that supplied in Drayson's books, when they are *studied*, and not merely raced through under the influence of an obsession that their author had merely hit upon an empirical formula for calculating the obliquity. See later remarks under the heading of 'Certain Consequences.'

* It will not have escaped notice that whereas, as pointed out on p. 9, in Paragraph Four *i* the pole of the Invariable Plane, 1½° from the pole of the ecliptic, is implicitly assumed as the centre of the precessional polar motion, Paragraph Eight on the other hand implicitly maintains *e* the pole of the Ecliptic as the centre,—a shifting of the grounds of argument which appears to be unconscious on the part of the author.

The Proper Motion of the Stars.

It will be remembered that in commenting on Paragraph One this subject was alluded to, and its consideration deferred.

'At first,' we read, 'Drayson maintained that the fixed stars had no proper motions at all. He claimed by his data to be able to compute the R.A. and Dec. for any star in the past or future from one observation of that star. This he clearly could not do, if there were any proper motion.' In this remark we have a sufficient proof of the critic's entire failure to apprehend the nature of the problem to be discussed; and it calls for a little further exposition.

What is the proper motion of a star? By definition it is: A real movement of the star itself, as distinguished from any apparent movements caused by a displacement of its position due to the motion, either rotational or orbital, of the earth itself. That is the definition, in theory. In practice, however, this definition necessarily resolves into: Any residual discrepancy remaining between observed position and calculated position, *when all known causes of displacement have been duly allowed for.*—All known, or rather, to put it more accurately, all recognised causes of displacement.

If then, there should exist some *un*-recognised source of displacement, is it not clear that this must introduce a discrepancy between the calculated and the true position? And is it not clear also that, owing to the non-recognition of this unallowed-for element, such discrepancy will inevitably be ascribed to a 'proper' motion of the star itself?

Drayson's contention is, that this is what has occurred, and that the second rotation supplies the unknown element. Of the truth of this contention he supplies proof by calculating positions which agree with observation without recourse to any so-called proper motion.* To say, then, that he cannot do this 'if' there are any appreciable proper motions is a sheer begging of the question; for the very point to be decided is as to whether these supposed proper motions are real or illusory. One is a little inclined to wonder that this part of the subject was not selected for the demonstration of Drayson's fallacy.

Seeing however that our critic, whether of set purpose or not, has refrained from this demonstration it may be permissible to say that the present writer has investigated this aspect of the problem with some degree of thoroughness. A certain well known bright star, having an assigned proper motion of about half a second annually, was taken and its movement examined under the Draysonian conditions, applied with the utmost strictness. And it was found—after a rather lengthy investigation which led to discoveries of some interest—that these conditions enabled the observed motion of this star to be reproduced for a period of one hundred and sixty-five years with a total discrepancy, during the whole of that period, of *not more than one second of arc* either in Right Ascension or Declination. This demonstration has been committed to writing and shown to certain friends who will doubtless be prepared to testify to its correctness when they have verified the actual figure-work.

* These have been done publicly, *e.g.*, *Royal Engineers' Journal*, vol. xxiv, p. 247, November, 1894.

The investigation did not prove to be by any means plane sailing, for, as one can easily understand *must* be the case, if Drayson is right, the persistence of an erroneous concept of polar motion during so many centuries has led to anomalies in astronomical procedure, one of which is the error in the computation of time first pointed out by him in 1870 : (*M.N.R.A.S.*, vol. xxxi, p. 38). These anomalies have added to the difficulties of the problem, and they naturally render the demonstration somewhat elaborate and complex. It is an easier matter to get a skein into a tangle than to straighten it out, and the same is true of intellectual tangles.

For this reason one is naturally reluctant to bring this demonstration forward publicly until a capacity for more unprejudiced and—indeed one must say it !—more intelligent examination of the matter is exhibited by astronomers generally.

It may be said : ' But even accepting this result at the value claimed, what of it ? It is only a single instance, and one swallow doesn't make a summer.' To which one may reply : ' True. And if you want an excuse for escaping the issue you had better ride away on that ! Though don't forget that even one swallow wants explaining, and besides there are numerous instances given in Drayson's books and never disproved.' Those who put the question in a spirit not of captiousness but of honest doubt, may be referred to the consideration brought forward by the writer in a letter on page 101 of *Science Progress* for July 1920,* from which it will be seen that the general consensus of the stars supports the result just given : namely, that the proper motions of the stars are ILLUSORY.†

It is here that the magnitude of Drayson's discovery becomes apparent. If the alleged proper motions of the stars turn out to be unreal, it follows that our present conceptions of the size and shape of the universe, on which subject so much is now being written, must necessarily be largely if not wholly mistaken.

' But,' the reader may again break in, ' we know, indisputably, by spectroscopic evidence that the stars are in motion.'

Do we, indeed ? Let us not forget that spectroscopic investigations on these lines were commenced in the first instance with a very strong prepossession in the mind of the investigators in that direction, founded upon prevailing astronomical belief created by inferences (mistaken inferences if Drayson is right) derived from observed positions. Besides there is the motion of the earth to be taken into account, and of the sun too perhaps.

' Oh, but that is taken into account,' replies the well-informed reader, eagerly ; ' see Chapters IV and V of Dr. Campbell's *Stellar Motions*.'

Yes. But see also the end of the second chapter of that very interesting book. And remember, too, that the position of the Solar Apex enters as a necessary factor into all these determinations. This Apex of Solar Motion is shown in the letter to *Science Progress*, already mentioned, to be illusory.

* Given later in the Appendix. See also *Warmer Winters*, p. 19.

† Since this sentence is possibly open to misinterpretation let me add that Drayson did not assert (nor do I) that the stars have or can have no motion. All that is asserted is, that the ' proper motions ' *at present assigned* do NOT result from any motion of the stars but from a motion of the earth improperly conceived.

Whence follows the consideration that, since this point when rightly interpreted affords no evidence that the Sun's Motion in Space is in that direction at all, the said motion, if any, may take place in *some altogether different direction*, requiring a new determination both directional and quantitative, and corresponding corrections to, or modifications of, all existing stellar velocities—radial as well as transverse. Parallaxes are possibly affected to some extent also, but may be left till these matters have been decided.

These are very serious considerations, amounting to no less than a complete revision of the whole of existing stellar data.

Independent Investigation.

The easiest way of dealing with this difficulty is to shelve it, to pooh-pooh the whole matter until positively forced into action by outside pressure, and up to the present orthodox astronomy appears to have preferred that course.

Now that attention has been awakened it may be hoped that there will be one or two adventurous spirits who will feel inclined to investigate for themselves. But they must not expect results to tumble into their laps. The present writer hopefully entered on his quest with the intention of examining the matter thoroughly if it took him a whole week to do so.—Seven days.

But the seven days became seven years before the last of his difficulties was surmounted and certainty of Drayson's correctness gained, although certainty of the *in*correctness of conventional procedure was reached at a very much earlier stage. The problem he was faced with was, to establish proof of the correctness of a certain movement, by an appeal to data obtained under a system dominated by a false conception of this movement, and therefore yielding figures which must necessarily be more or less false in themselves, although—since the false conception was not the movement of Nature—these figures must be capable, with proper handling, of being made to reveal the truth. That was the problem, and it had some analogy with that of an accountant to whom is given the task of discovering from the books of a business firm whether or not a suspected embezzlement, and consequent falsification of the books, has occurred. But this analogy does not completely hold, because here detection was rendered more difficult by the fact that in this case the falsification, if any, was entirely unconscious and involuntary instead of being calculated and deliberate. Enough that in the end the problem was solved.

The investigator should be prepared to work for a while as a student before expecting to don the robe of the judge. If this seem a hard condition, it will at least not deter anyone who has once apprehended the importance of the problem.

It may be useful to indicate briefly one or two of the difficulties by which the budding investigator is likely to be 'put off.' One of the first of these, probably, will be the comparison of ancient obliquities with Drayson's results by formula. The most ancient, and therefore likely to be the first tried, is one derived from the reports of the Jesuit Missionary, Father Gaubil, and quoted approvingly by Laplace in his article in the *Connaissance*

des Temps for 1811 as according so well with the gravitational theory.* Drayson's, for the same date, comes out *a whole degree* higher. Of this more anon.

Ancient Determinations of the Obliquity.

In general it may be said that when one comes to examine the accepted values for ancient obliquities one finds Drayson's somewhat higher.

Each case must be dealt with on its merits, but it may be pointed out that the evidence for *all* the ancient obliquities is such as we cannot examine at the present day. We have to take it or leave it, assuming such modifications as best accord with our special predilections. In all cases they are derived from the traditionary or recorded lengths of the shadow of some obelisk or gnomon, and neither the shadow nor the obelisk—nor even the original record—is present to undergo cross-examination, so that we are obliged to accept the advocate's statement. If the reasoning given in the letter to *Science Progress* for October 1920 be allowed,† we have incorruptible evidence of Drayson being entirely correct in his obliquity 5644 B.C., or at least fifty centuries earlier than he himself ever hoped to find evidence for. The letter itself must be consulted for details of the argument.

But whatever value may be attached to this as evidence the final issue will turn on the accumulated observations of stars during the past two centuries.

It may be wondered why such a glaring discrepancy between an accepted value for the obliquity in ancient times, and Drayson's value for the same epoch, should be openly pointed to by an advocate for Drayson. But if well weighed with what has gone before it will be seen that these statements of 'absent witnesses' who have not 'surrendered to their subpœnas,' or perhaps one should rather say on whom subpœnas cannot be served, are in direct conflict with the testimony of those witnesses who are ready at any time to undergo cross-examination—namely modern star observations: see, for example, the letter in *Science Progress* for July 1920. Between present and absent witnesses there can surely be but one choice where the testimony is of opposite character?

Coming down from generalities to actual figures, it will be instructive here to recur to the three obliquities used in Paragraph Six, in commenting on which it was remarked that the test aimed at would have been better secured by assuming the decrease in the angle *PEZ* constant at its present rate, which is the precessional rate of 50″·25 a year. Working this out we find (using 5-fig. logs.) the following values, which we can compare with the previous table :—

Year	*O.E.*	*PEZ*	*CP*	*CE*
1890	23° 27′ 12″·79	5° 39′·19	29° 25′·9	6° 0′ 0″
1755	23° 28′ 17″·6	7° 32′·26	29° 25′·74	6° 0′ 0″
140	23° 50′ —	30° 4′·81	29° 10′·3	6° 0′ 0″

From this it can be seen that when the correct or *nearly* correct value

* How ironical this sounds in the light of Sutcliffe's discovery a century later!

† Given later in the Appendix.

of *PEZ* is used *CP* is virtually constant, and identical with Drayson's value namely 29° 25′ 47″ (instead of 29° 26′·3) during a century and a half, but that it is—apparently—hopelessly far out when we go back twenty centuries,

Now let us work the test the other way about. Assuming, as before, *CE* to be 6° 0′ 0″, let us take Drayson's value for *CP* as just given, and his values for *PCZ*, namely those erroneously given for *PEZ* in Paragraph Six as already explained ; and let us find what obliquities we obtain for the three dates. Seven-figure logs. are needed, five-figure not being sensitive enough to obtain accuracy to seconds.

Year	*O.E.*	*PCZ*	*CP*	*CE*
1890	23° 27′ 12″·35	4° 35′·30	29° 25′·78	6° 0′ 0″
1755	23° 28′ 18″·9	6° 7′·13	29° 25′·78	6° 0′ 0″
140	24° 5′·1	24° 25′·63	29° 25′·78	6° 0′ 0″

Here then we see complete accordance with observed O.E. for 1890 and 1755, the discrepancy of 1″·3 being inconsiderable when all the circumstances are taken into account, since the accepted values vary more than this. Yet we find a difference of 15′ in the value for 140 A.D., which looks serious.

But it looks a good deal less serious when we begin to enquire how the accepted value of 23° 50′ has been arrived at.

In the first place Ptolemy, who is the authority, in his Catalogue of Stars never gives the position of a star more nearly than to the sixth of a degree. And if he did not attempt higher accuracy than that with a very easily observable object such as a bright star, he certainly could not achieve it beyond question in respect of the sun at the solstice, which in all respects is a more difficult observation so far as precision is concerned. Indeed it is misleading even to print it as 23° 50′—it should be given as 23°·8 or 23°·9 or even 24° roundly. Baily in his edition of Ptolemy's Catalogue sins in this respect, printing for instance 5° 50′ instead of $5\frac{5}{6}°$ as he should have done. For the *mental effect of the printed digits is very strong even after you have been warned* in the preface that *they are not intended to imply an accuracy greater than one-sixth of a degree.*

Ptolemy's Obliquity for 140 A.D.

What Ptolemy himself says (*Almagest*, bk. i, ch. 12) is that from a number of shadow observations of a kind which he describes, taken very carefully, ' we have found ' that the difference between the tropics (*i.e.*, twice the O.E.) falls between the limits $47°\frac{2}{3}$ and $47°\frac{3}{4}$; and he goes on to remark with satisfaction that this amounts to about the same value as that determined by Eratosthenes (250 B.C.) and used by Hipparchus (140 B.C.), ' which was eleven eighty-thirds of the circle.' Ptolemy's statement therefore implies an accuracy of about one-twelfth of a degree, or 5′, whereas that of Hipparchus, which he quotes approvingly as supporting his own determination, implies an accuracy of only about one eighty-third of the circle or say 4°. Delambre contends with some amount of reason that Ptolemy was not himself an observer at all, but that he merely instanced determinations current in his day, in the way that a writer of a text book—which is what he was—naturally would do, and as our own ' authorities ' do to-day (*e.g.*, Newton, Laplace).

The testimony of Strabo concerning the well at Syene is valuable. Syene, he says, stands immediately under the tropic ; a well is sunk here, which marks the summer solstice, and the day on which it happens is known when the style of the sundial casts no shade at noon. At that instant the vertical sun darts his rays to the bottom of the well, *and his entire image is described upon the water.** Now it is noteworthy that Ptolemy in the *Almagest* assigns Syene (Assouan) a latitude of only 23° 51′, or 15′ less than it should have according to modern determinations. Moreover he makes the latitude of his own city Alexandria 30° 58′, whereas it should be 14′ more (*Almagest*, ii, 6, v 12, 13). These two facts taken together appear to indicate that there is warrant for concluding that the obliquity may have been as high as 24° 5′ in Ptolemy's day.

Certain observations in the *Almagest* afford independent corroboration of this, but the proof which may perhaps be published later is too long to include here. Yet this much may be said : that a further investigation pursued since this Reply was in type and ready for the printer, has resulted in showing that it is quite possible to reconcile ALL THE CHIEF ANCIENT OBSERVATIONS of the obliquity with Drayson's figures, though not the one now to be mentioned.

The ancient Chinese obliquity already referred to as praised by Laplace assigned by chronologists to 1100 B.C., is of a summer solstice shadow of $1\frac{5}{10}$ feet, and a winter solstice shadow (less trustworthy) of 13 feet, the gnomon being 8 feet in height, latitude of place not quite certain but about $34\frac{1}{2}°$. The curious reader can calculate—as did Delambre—the discrepancies resulting by assuming an error of plus-minus 1/100 foot in the shadow. But this observation comes into an entirely different category from Ptolemy's, for to reconcile it with Drayson the improbable error of nearly two inches in the shadow must be supposed. Delambre, after comparing this with other Chinese records of later date (*Astr. Anc.*, i, 391, 392), rejected it as altogether unreliable, concluding that ' Il resulte malheuresement de cet exposé, qu'on ne saurait tirer aucune lumière certaine des longs travaux des Chinois. . . . Ne pourrait-on pas craindre qu'on n'ait attribué a Tcheon-Kong des ombres (*solsticiales*) observées 1300 ans plus tard, et cela dans la vue de rendre l'Astronomie chinoise plus respectable par son ancienneté ? '

We learn from all this that when ancient obliquities are *enquired into*, one finds that they may as plausibly be maintained as supporting (or at least not contradicting) Drayson, as confuting him—according to the preferences of the enquirer ! And so we are brought back to our former remark, that *the final issue must lie with the star observations of the past two hundred years.*

A Test which the Reader can make for himself.

Yet there is one little test, confirmatory in its way, which the investigator can make with the figures already given. Taking Drayson's triangle *PCE* for 140 A.D., the calculated value of *PEZ* is found to be 29° 51′·74.† This

* Lib. xvii, quoted in Kenealy's *Book of God*, p. 194.

† The value 30° 4′·81 on p. 21 is obtained, remember, on the assumption that the C.E. was then exactly 23° 50′.

angle divided by $2154\frac{3}{4}$, the years intervening between 140 A.D. and the year $2294\frac{3}{4}$ A.D. when *P*, *C*, *E* are in alignment, gives 48″·89 as the average annual decrement in *PEZ* during that time. The decrease in this angle, it will be seen, measures the precessional movement of the solstitial colure (represented by the arc *PE*) round *E* the pole of the ecliptic. This movement is not uniform, but, as is obvious from the diagram, is subject to a gradual acceleration as *P* moves towards *Z*. Remember now the statement quoted in a footnote a few pages back, that Hipparchus estimated the annual precession at 48″, which *was* its rate then according to Drayson, whereas the present rate is about 50″·25. The mean between the two is 49″·12, which is almost exactly the *average rate* just calculated and therefore seems to confirm both Drayson and Hipparchus, the one by the other. Or else, it is a curious coincidence.

A more striking test will be found in the Appendix (VI).

'Certain Consequences.'

At the end of the comments on Paragraph Four (p. 9) it was said that 'certain consequences' flow from Drayson's polar movement, and a further reference to these was promised. One of them is of course the succession, and dates, of the geological Ice Ages. But the one chiefly in mind was the effect of the Second Rotation on the zeniths of the various observatories of the world, whereby each zenith moves annually in reference to the stars, in a manner not understood and not allowed for in current astronomical procedure. That omission is really culpable on the part of astronomers, in view of the fact that it was pointed out to them by Drayson half a century ago, and more especially in view of the further fact that, *even if Drayson's centre were not the true centre of polar motion and E, the pole of the ecliptic, were that centre as it is asserted to be*, this unrecognised movement of every zenith would still occur, as a result of the Second Rotation; and that it would be different as regards each star, for the same observatory, and different also as regards the same star for any two observatories. Although it is true that since these differences depend, essentially, on the latitude of the observatory, and since the chief observatories of the world are all comprised within a comparatively narrow belt of latitude, that difference has not hitherto become obtrusive enough to provoke investigation—except in the sense that (in a diffused form) it has given rise to the so-called Latitude Variation of which one hears so much.

'As a result of the "Second Rotation,"' chimes in some critic, scoffingly; 'But astronomers do not admit any second rotation, and the onus is surely on you to prove your second rotation before you ask astronomers to investigate the hypothetical consequences of an apochryphal movement?'

So astronomers 'do not admit' any second rotation? Possibly. But if so they must have given strangely little thought to the matter. Indeed, they must be oddly ignorant of what their own authorities have stated. In Sir John Herschel's *Outlines of Astronomy* is a statement showing that he at any rate recognised that such a second rotation did take place, although it is true he made as its axis the pole of the ecliptic. The reference will be

found in Article 909 of the *Outlines*. That Article if studied attentively will show that, even on the accepted theory of the pole of the ecliptic being the centre of the precessional movement, a 'second rotation' does undoubtedly occur. And it is rather strange that its writer did not stop to think that this movement must inevitably affect every zenith on the earth, annually, in its relation to the Right Ascension and Declination of a star, in a manner analogous to that in which the *daily* rotation affects the Azimuth and Altitude of a star.*

Drayson's Alleged Change of Opinion.

Referring back to the last sentence of Paragraph One, and comment (p. 3), it is true that in one of Drayson's letters to Admiral de Horsey he says of a certain star (Alpha Centauri) 'it is one of the few stars which I believe really *has* a proper motion.'

But in the light of the writer's researches already referred to it seems likely that had the methods he has employed been used by Drayson, even this star might have shown no proper motion.

The reason for making the suggestion is this. The bright star previously mentioned is one included in Drayson's earlier researches,† and it happens that when the motion there given is examined over a longer period of years than is there done, a small discrepancy is noticeable. This discrepancy however disappears when the writer's method—which may be described as a somewhat stricter or more elaborate application of Drayson's own—is employed. It is impossible to indicate its exact nature here, but for the purpose of illustration it may be said to correspond roughly to the taking into account of 'second differences' when using the Nautical Almanac, whereby a slightly more accurate result is attained than when, as commonly, such differences are ignored. It does not seem unreasonable therefore to suppose it probable that by the employment of the same method a like removal of the discrepancy might result in the case of Alpha Centauri.

SUMMARY AND CONCLUSION.

So much ground has necessarily been covered in the course of a Reply dealing with the many points raised, explicitly or implicitly, in this professed exposure of the 'Draysonian Fallacy,' that a short Summary seems called for, or rather, perhaps, a

Statement of the Position.

1. It has been shown, in the first place, that the accepted doctrine of the cause of precession postulates a movement of the pole along a certain path which it has not been proved by astronomers to follow—they just took it for granted—and which Drayson proved, or claimed to have proved, that it did not take. This anomaly is the kernel of the whole issue, and until it is recognised and treated as such, no headway is possible in any discussion. (pp. 1, 2.)

* See letter quoted in the Appendix, on "Wegener's Theory and Greenwich Meridian."

† *Untrodden Ground*, p. 171.

2. Drayson's polar motion suggested a date for the last Ice Age so recent as to be altogether rejected by the geologists of his time, though since then evidence has accumulated, and is still accumulating, which establishes complete harmony between the facts of geology and the new astronomical data. (p. 2.)

3. The description of Drayson's system in Paragraph One, although fair and accurate as far as it goes, omits an important element. (p. 3, and later, p. 17.)

4. Drayson was the first astronomer to connect the phenomena of 'precession' and of the 'decrease of the obliquity' as due to *a single cause.* Hitherto this had not been done, owing to the idea that the plane of the ecliptic had shifted its position with reference to the stars during historic time, which change being attributed to gravitational causes affecting the earth's orbit and not its axis, the two phenomena were regarded as unrelated. This, notwithstanding the fact that if the pole of the ecliptic were indeed the *centre* of the polar movement the obliquity could not decrease, since the angular distance of these two poles is by a geometrical necessity the measure of the obliquity, and the centre of a circle does not decrease its distance from the circumference; whereas, observation had shown that the obliquity did decrease. Drayson, having re-examined the alleged evidence for change in the position of the ecliptic found it unsatisfactory and accordingly rejected it, treating the plane of the ecliptic as sensibly invariable and its pole as fixed during historic time. (pp. 4-6.)

5. The accepted explanation of the cause, and limits, of the variation in the obliquity shown to be untenable because vitiated by a blunder of Laplace, which was not discovered till 1905, more than a century later, and has since remained unacknowledged. (pp. 6-8.)

6. It having become evident by the decrease in the obliquity that the pole of the ecliptic could not be the centre of the pole's circular path, the problem as to what that centre might be, or whether indeed the path were circular, remained unattempted until Drayson attacked and solved it, with unexpected consequences. (pp. 8, 9.)

7. (*a*) Drayson's assumed ignorance of the 'circle of curvature' and its properties.

(*b*) The art of exposing a fallacy. Our critic's misrepresentation or rather blunder, and how the reader may prove for himself that it is one. (pp. 9-12.)

(*c*) Some climatological points. (p. 12-14.)

8. The Newtonian explanation of precession and its underlying assumptions briefly examined, with the bearing of geodesy on the question. (pp. 14-16.)

9. The author has performed a service to astronomy in issuing his Paper. His attitude characteristic of the class he represents. (p. 17.)

10. The omitted element in Paragraph One. The Second Rotation and its astronomical importance. (p. 17 and ref. pp. 24, 25.)

11. The proper motion of a star. What it is in theory—a real movement of the star ; and what it is in practice—a discrepancy between calculated and observed position. Significant result of a personal investigation of this matter by the writer. (pp. 18, 19, and ref. p. 25.)

12. Magnitude of Drayson's discovery. (p. 19.)

13. Spectroscopic evidence and the Apex of Solar Motion. (pp. 19, 20.)

14. Independent investigation hoped for. Apparent disproof at the very first test. (p. 20.)

15. Ancient determinations of the obliquity. Re-investigation of our critic's test based on the obliquity in 140 A.D. (pp. 21, 22.)

16. Facts tending to show that there is strong probability that the obliquity in Ptolemy's day was as high as Drayson requires. Testimony of Strabo. Independent proof from the *Almagest*. Result of an investigation made by the writer since this Reply was written. (pp. 22, 23.)

17. Conclusion from ancient obliquities equivocal. Final result lies with modern star positions of last two centuries. A simple test available to the reader. (pp. 23, 24.)

18. Unrecognised movements of the zeniths of observatories. Herschel's implicit endorsement of the fact in recognising the existence of a second rotation. (pp. 24, 25.)

19. Drayson's alleged change of opinion, and the possible bearing on this of the writer's investigation already mentioned. (p. 25.)

The astronomical reader is asked to assume for a moment the truth of the claim here made,—to imagine that Drayson was right, that his discovery was a real discovery and its acknowledgment merely a matter of time, and that, accordingly, current astronomical teachings on all matters thereby affected are erroneous. Giving the imagination full play, let him reflect on what the consequences may be.

Neglecting entirely the geological aspect of the question, as foreign to our purpose, we have the following prospect. Proper motion of the stars not existing, or being infinitesimal, two results ensue, one practical, the other (far more important) intellectual and philosophical. Firstly, their positions can be calculated and much if not most of the perpetual observation now deemed necessary will be eliminated. *Is* it really necessary ? No ! For what should we think of the astronomy which required to observe four or five altitudes of the sun at ten-minute intervals, in order to predict its position at the end of the following hour ? Such ineptitude it seems impossible to imagine ! Yet this would undoubtedly be necessary if astronomers laboured under the impression that the pole of the heavens was six degrees or so from its real position and made their calculations accordingly.* This illustration is very important, for it supplies a fair analogy with our

* Try it yourself. Add 6° to the latitude of your place of observation, work out the altitudes in the ordinary way, and compare with observation !

procedure regarding the stars. Not knowing the true pole of their annual movement, discrepancies occur between calculated and observed position, and, like a careless housewife short in her accounts, we call the deficit 'Sundries'—*id est*, PROPER MOTION!

The second result is this. The proper motions at present assigned being illusory,* and the so-called Apex of Solar Motion a fallacy, all schemes of cosmical evolution or estimations of the size and shape of the sidereal universe, into which these must necessarily enter implicitly or otherwise as essential factors, are necessarily deceptive and unreal. And the more elaborated they become, the farther shall we wander from the truth, for each new attempt will but enlarge the bounds of error.

The state of our Modern Astronomy is perhaps not so flourishing as—its trumpeters! To those who look below the surface, its vaunted precision is seen to be rather instrumental than theoretical (using this word in its true sense); for everywhere there is non-accordance, discrepancy, variation—discrepancies and variations small indeed, and yet significant because unaccounted for. Hear Simon Newcomb: 'The small outstanding differences are *greater than can result from the chance errors of individual* observations, thus indicating that *hidden sources of error still exist.* A classification of the errors with respect to the positions of the stars, shows that they are mainly a function of this element. . . . . In framing the positions of the stars one of the efforts of the astronomer will be to look for and eliminate *every cause* which can introduce into his determinations errors depending on the position of the stars.'† (Italics added.)

The pertinency of this extract will be appreciated, in the light of the remarks on Zenith Movements: (pp. 24, 25). For although since it was written the so-called phenomenon of 'Latitude Variation' is held to have been established, it will be noticed that this affords no explanation for, but merely a palliation of, discrepancies which it affects rather to excuse than to explain. How, too, happens it that despite the patrolling of this armed and mounted Latitude-Variation a certain bright star beforementioned (p. 18) went on its way all undismayed and kept its Draysonian appointment after 165 years?

A digression perhaps. To return. The incessant publication of 'corrected' or 're-determined' star-positions, elements, constants, etc., ostensibly in the cause of increased accuracy, by maintaining a fevered procession of 'new values' serves rather to disguise the symptoms than to cure the ill—like changes of physic in treating a chronic disease. The newer value is not necessarily the *truer* value, but the latest or most 'correct' fashion. And the result of the whole process is, in practice, that comparisons of existing observations at widely separated dates involve such an immense amount of labour (and also, mark, risk of numerical error) in the preliminary co-ordination, that far less investigation of the kind is done, and of that little the probable value is far less, than might be.

† See previous footnote on p. 19.

* *Fundamental Stars*, 1867, p. 5. Read in this connection the letter quoted from the *Yorkshire Post*, at the end of Appendix VI.

'Rather alarming, this! All our star-places in *Whittaker* are wrong then, I suppose?' says a listener.

No. That is not meant. The 'discords' just referred to, though disquieting are not grievous. Disquieting, because they multiply. Not grievous, for they are small, so small that our forefathers would not have noticed them. But we live in a later age and our astronomical 'ears'—our splendid telescopes—are becoming more acute, they perceive them as discords.

'But you say the discrepancies, though small, are multiplying. Surely that affords rather a hopeless outlook?'

Not necessarily. Paradoxical though it needs must sound, the writer's researches incline him to the view that some of the fundamental items of astronomy are in reality more accurately known than is at present supposed; for example, the length of the year, and the mean motion of the moon. It is not possible here even to outline the grounds of this opinion, held tentatively as yet, but becoming stronger as fresh indications of its truth arise. The annual error in the computation of time, discovered by Drayson (and of which the writer considers he has been fortunate enough to establish a demonstration, implicit yet formal),—this error of seven-tenths of a second annually, small though it is, is competent to account for discrepancies, discrepancies which, being unaccounted for, that is to say *misunderstood*, are remedied by 'corrections' which themselves introduce further error, just as the suppressive treatment of a sloughing ulcer drives the poison inwards and aggravates the disease. The supposed acceleration of the moon's mean motion, now universally regarded as an established fact of science—as was the *retrograde* motion of the moons of Uranus, until Drayson exposed that error, which is no longer countenanced—is probably another case of the same kind: one might say 'undoubtedly,' but that a quantitative investigation has not as yet been attempted. This may seem a preposterous suggestion. How can the two things be related, one may ask? But, as novel readers have so often found, there may be an easily understandable explanation of an apparently impossible relatedness, that simply hadn't happened to occur to them! Drayson often expressed his admiration for the reasoning powers of the Zulus, who, he found, could surpass some astronomers in power of drawing inferences from observed facts.* Perhaps he may have gained something, some new way of contemplating facts it may be, from his sojourn among them.

The references given should be looked up in order to appreciate the exact significance of this allusion, which has a less general application to-day perhaps than when Drayson wrote. His point was that the Zulus were habituated from birth to observe accurately and to draw correct deductions from their observations, whereas text-book instruction and the examination system in vogue during the past century so far from developing fully these faculties tends to dwarf and inhibit them. And that he was *right* in this view is implicitly recognised in the existence of the Boy Scout movement to-day.

The darkest hour precedes the dawn. The present stage of astronomy,

* *Experiences*, p. 131; *New Science Review*, January, 1895.

regarded from this standpoint, is pregnant with hope. The like has been known before, and the fact affords warrant for anticipating an era of progress on a scale and of a magnificence beyond our most ardent desires.

In the Middle Ages, towards the commencement of the sixteenth century, the condition of astronomy was that of a work-basket with a kitten in it—a hopeless tangle. Milton's lines from *Paradise Lost*—

> 'the sphere
> With centric and eccentric scribbled o'er,
> Cycle and epicycle, orb in orb . . .'

well convey the immeasurable confusion into which the ceaseless efforts of industrious men striving to reduce to system the rapidly accumulating results of far more numerous and better co-ordinated observations than had ever before been available, had reduced it. And why? Because they were working on a *false basis*; trying to express sun-centred movements in terms of earth-centred mechanism. It was like trying to solve a Latin cipher in the belief that it had been written in English.

Copernicus—Galileo—Kepler—Newton. . . . . Modern Astronomy.

Would it be possible now to return to the system of Ptolemy, which held its place unchallenged for fifteen centuries? No answer is required.

There is no need to labour the point. The analogy is sufficiently clear. The Copernican *change-of-centre* heralded a new concept of the Solar System. And not only an astronomical, but a philosophical and in the end a moral and social revolution resulted. For change in thought precedes change in action, and has many and ramifying results. Revelation and revolution are but as cause and effect.

May it not be just possible that the Draysonian *change-of-centre* presages a new concept of the Sidereal System, and all the door-opening in the worlds of thought that that implies?

Let those who are inclined to reject this suggestion offhand, ask themselves how they would regard a medieval astronomer who refused to contemplate the possibility of the well-tried and scientifically-proved system of his day—the '*admitted deficiencies*' of which were being daily 'brought more closely into accord with refined modern observation by newer and more powerful mathematical methods,' (*id est*, epicycles and eccentrics)—who refused to contemplate the possibility of this system giving way to one which seemed to deny its fundamental conceptions? Yet it did. And simplicity resulted.

Now we have again reached complexity. The time is ripe for another revelation.

Imaginative? Perhaps. And yet from the mistakes of the past one may learn lessons for the future.

If there should be any student of astronomy who, reading our critic's Paper once more, in the light of the distinction between 'formal' and 'physical' astronomy to which attention has been drawn, feels the matter to be one not for argument or controversy but for personal investigation and discriminative study—well, if he cares to make application through the Publishers, the writer will be glad to put him in the way of obtaining evidence on the subject and, indeed, of proving for himself, by the kind of evidence declared impossible by our critic, whether Drayson was right.

AUTHOR'S NOTE.

What led to the writing of the foregoing tract was a paper read before the British Astronomical Association and subsequently published in their Journal in the beginning of the present year, under the title of "The Draysonian Fallacy."

Having made a close study of the problem associated with the name of Drayson, and thereby,—following in the wake of Admiral Sir Algernon de Horsey, K.C.B., who did likewise, and whose correspondence with Drayson I had had the benefit of reading,—having after full enquiry and persistent endeavour satisfied myself that Drayson was right, I naturally felt it incumbent on me to examine this paper and point out wherein it was at variance either with facts which I had been at pains to discover, or with conclusions which were the outcome of a seven years' investigation.

The first seven paragraphs of the Reply, together with its last, formed part of a letter written to Major Marriott, whose pamphlet, "Warmer Winters," in which the geological and climatic consequences of Drayson's discovery are summarised, had so evidently given rise to this astronomical rejoinder. But geological as well as astronomical issues had been raised, and, as it was so manifestly his own writings which were pointed at, Major Marriott although not a member of the Association craved space in the "Journal" for a reply. He asked if he might append my remarks, and to that I willingly agreed, though I thought fit to withhold my name.

It occurred to me however on reflection that although these seven paragraphs really go to the root of the whole question, *a detailed examination and Reply should if necessary be forthcoming, and it was accordingly prepared.*

Since it was written, I learn that the letter referred to has been refused publication on the ground that "the Council is unable to accept an anonymous contribution communicated through a non-member;" which seems unnecessary insistence on a point of etiquette where a matter of scientific importance is at stake. Publication of the full Reply, which else might have been waived, has thus become imperative.

In an article of this kind, it seems to me, the identity of the writer does not—or should not—concern the reader. The knowledge that N or M has written it can be of no help in judging of the soundness of its arguments but rather the contrary, as introducing a probably unconscious bias either for or against. Anonymous articles are justly condemned when containing important statements dependent for their credibility or significance on the trustworthiness of the narrator, or if they contain personalities, in either of which cases the name of the writer would require to be known. Here that is not so, and I consider that a pen-name might have been preferable. The avowal of my attachment to astrology may not be necessary perhaps, but had not my interest in the subject of precession been awakened, and kept vivid, through its connection with my astrological *studies, I should never have troubled to undertake the researches to which it has led me.*

ALFRED H. BARLEY,

Sub-Editor of "Modern Astrology," 1903–1917.

LEPPINGTON HOUSE, HERTFORD.
25–8–22.

"*The witnesses may commit perjury, but the smoke cannot.*"

ABRAHAM LINCOLN:
(Letter to J. R. Underwood, October 26, 1864.)

APPENDIX.

I.

ON THE RELATION BETWEEN FORMAL AND PHYSICAL ASTRONOMY.

Being the Introduction to Book V of the 'History of the Inductive Sciences,' by William Whewell, Master of Trinity College, Cambridge. (Ed. 1857.)

We have thus rapidly traced the causes of the almost complete blank which the history of physical science offers, from the decline of the Roman Empire, for a thousand years. Along with the breaking up of the ancient forms of society, were broken up the ancient energy of thinking, the clearness of idea, and steadiness of intellectual action. This mental declension produced a servile admiration for the genius of the better periods, and thus, the spirit of Commentation: Christianity established the claim of truth to govern the world; and this principle, misinterpreted and combined with the ignorance and servility of the times, gave rise to the Dogmatic System: and the love of speculation, finding no secure and permitted path on solid ground, went off into the regions of Mysticism.

The causes which produced the inertness and blindness of the stationary period of human knowledge,* began at last to yield to the influence of the principles which tended to progression. The indistinctness of thought, which was the original feature in the decline of sound knowledge, was in a measure remedied by the steady cultivation of Pure Mathematics and Astronomy, and by the progress of inventions in the Arts, which call out and fix the distinctness of our conceptions of the relations of natural phenomena. As men's minds became clear, they became less servile: the perception of the nature of truth drew men away from controversies about mere opinion; when they saw distinctly the relations of *things*, they ceased to give their whole attention to what had been *said* concerning them; and thus, as science rose into view, the spirit of commentation lost its sway. And when men came to feel what it was to think for themselves on subjects of science, they soon rebelled against the right of others to impose opinions upon them. When they threw off their blind admiration for the ancients, they were disposed to cast away also their passive obedience to the ancient system of doctrines. When they were no longer inspired by the spirit of commentation, they were no longer submissive to the dogmatism of the schools. When they began to feel that they could discover truths, they felt also a persuasion of a right and growing will so to do.

Thus the revived clearness of ideas, which made its appearance at the revival of letters, brought on a struggle with the authority, intellectual and civil, of the established schools of philosophy. This clearness of idea showed itself, in the first instance, in Astronomy, and was embodied in the system of Copernicus; but the contest did not come to a crisis till a century later, in the time of Galileo and other disciples of the new doctrine. It is our present business to trace the principles of this series of events in the history of philosophy.

I do not profess to write a history of Astronomy, any further than is necessary in order to exhibit the principles on which the progression of science proceeds; and, therefore, I neglect subordinate persons and occurrences, in order to bring into view the leading features of great changes. Now in the introduction of the Copernican system into general acceptation, two leading views operated upon men's minds; the consideration of the system as exhibiting the apparent motions of the universe, and the consideration of this system with reference to its causes; the *formal* and the *physical* aspect of the Theory; the relations of Space and Time, and the relations of Force and Matter. These two divisions of the subject were at first not clearly separated; the second was long mixed, in a manner very dim and obscure, with the first, without appearing as a distinct subject of attention; but at last it was extricated and treated in a manner suitable to its nature. The views of Copernicus rested mainly on the formal condition of the universe, the relations of space and time; but Kepler, Galileo, and others, were led, by controversies

* Book V, to which this is the Introduction, is entitled "History of Formal Astronomy after the Stationary Period."

and other causes, to give a gradually-increasing attention to the physical relations of the heavenly bodies; an impulse was given to the study of Mechanics (the Doctrine of Motion) which became very soon an important and extensive science; and in no long period, the discoveries of Kepler, suggested by a vague but intense belief in the physical connexion of the parts of the universe, led to the decisive and sublime generalisations of Newton.

The distinction of *formal* and *physical* Astronomy thus becomes necessary, in order to treat clearly of the discussions which the propounding of the Copernican theory occasioned. But it may be observed that, besides this great change, Astronomy made very great advances in the same path which we have already been tracing, namely, the determination of the quantities and laws of the celestial motions, in so far as they were exhibited by the ancient theories, or might be represented by obvious modifications of those theories. I speak of new inequalities, new phenomena, such as Copernicus, Galileo, and Tycho Brahe discovered. As, however, these were very soon referred to the Copernican rather than the Ptolemaic hypothesis, they may be considered as developments rather of the new than of the old Theory; and I shall, therefore, treat of them, agreeably to the plan of the former part, as the sequel of the Copernican Induction.

II.

DANGER OF SUPERSEDING GEOMETRICAL BY ANALYTICAL METHODS.

(See Page 8.)

Drayson was not alone in this view. 'By seeing the subject presented only in an analytical form,' wrote Whewell in 1846, 'the student obtains a very erroneous notion of the relation of the subject to Geometry, and remains utterly ignorant of that part of mathematics which all mathematicians up to the present time have understood by the name of *conic sections*; . . . . students who have never become acquainted with the reasonings of conic sections in a geometrical form, will unavoidably have a very confused and imperfect comprehension of the reasoning of the *Principia* of Newton.'

The author of that great work himself had foreseen the danger: 'He (Newton) often censured the handling of geometrical subjects by algebraical calculations, . . . he frequently praised Slusius, Barrow, and Huygens, for not being influenced by the false taste which then began to prevail . . . . and even censured himself for not following them yet more fully than he did; and spoke with regret of his mistake at the beginning of his mathematical studies, in applying himself to the works of Descartes and other algebraic writers, before he had considered the Elements of Euclid with that attention which so excellent a writer deserves.' (*Life of Newton*, prefixed to his *Principia*, Motte: 1803, vol. i, p. xlix.)

And yet it is said to have been the boast of Laplace that *no diagrams* would be found in his book!

III.

NOTE ON PAGE 2—"THE FIRST TIME."

On page 2 the statement is made that the criticism answered in this reply marks the *first time* that the objections of astronomers have thus publicly, and in a semi-official way, been summarised. It may therefore be proper to mention the somewhat caustic review of Major Marriott's *Change in Climate* by the Rev. M. Davidson, B.A., M.Sc., F.R.A.S., which appeared in *Knowledge* for July, 1915, and which was very ably answered by the author in the following issue. Having read the article through afresh, before writing this note, the present writer can say that in no essential point does it differ from what has been dealt with here, and that Major Marriott's reply was more than adequate.

IV.

A SHORT BIBLIOGRAPHY.

Drayson's works are unfortunately now out of print, but they can be seen (or some of them) at any good Reference Library, also Mudie, and the London. They are here given in chronological order:—

Common Sights in the Heavens, 1862. (Chapman and Hall.)*

The Cause, Date, and Duration of the Last Glacial Epoch, 1873. (Chapman and Hall.)

The Cause of the Supposed Proper Motion of the Fixed Stars and an Explanation of the Apparent Acceleration of the Moon's Mean Motion, with other geometrical problems in Astronomy hitherto unsolved: (A sequel to the *Glacial Epoch*). 1874. (Chapman and Hall.)

[Articles in the *Quarterly Journal of Science*, edited by (Sir) William Crookes, 1874, 1875.]

Experiences of a Woolwich Professor during Fifteen Years at the Royal Military Academy, 1886 (Chapman and Hall). Gives an account of his discovery and the reception it met with, and much other interesting matter illustrative of the psychology of the official mind.

Thirty Thousand Years of the Earth's Past History, read by the aid of the Discovery of the Second Rotation of the Earth, 1888. (Chapman and Hall.) A new item, not in his earlier books, appears on page 63.

Untrodden Ground in Astronomy and Geology, giving further details of the Second Rotation of the Earth and of the Important Calculations which can be made by the aid of a knowledge thereof. 1890. (Kegan Paul.)

[Articles in the *New Science Review* (Philadelphia), 1894, 1895.]

Important Facts and Calculations for the Consideration of Astronomers and Geologists, 1896 (pamphlet).

[Correspondence, etc., in the *Royal Engineers' Journal*, 1894, 1898, 1899, 1900, 1901.]

Untrodden Ground in, etc., a Review, by Capt. W. Usborne Moore, R.N., 1900. (Charpentier and Co., 46 High Street, Portsmouth.) Professes to disprove Drayson.

[Drayson died 27th September, 1901.]

Draysonia; being an attempt to explain and popularise the system of the Second Rotation of the Earth as discovered by the late Major General A. W. Drayson, F.R.A.S., . . ., from which any person of ordinary mathematical ability is enabled to calculate the obliquity of the ecliptic, the precession of the equinoxes, and the right ascension and declination of the fixed stars for any date, past, present, or future, by Admiral Sir Algernon F. R. de Horsey, K.C.B. 1911. (Longmans.) An admirably concise and clear exposition, containing various useful tables and diagrams, and not seriously impaired by two or three 'slips' such as that in the second paragraph of page 21. The more thorough a study the reader makes of it, and of the whole question, the higher opinion he will have of this book.

The Change in Climate and its Cause, by Major R. A. Marriott, D.S.O., late Royal Marine Artillery, 1914. (E. Marlborough and Co., 51, Old Bailey, E.C., price 1s. 6d.) Contains an excellent astronomical summary.

The Glacial Epoch Explained, by R. A. Marriott, 1914. Reprint from the *Candid Quarterly Review*. (W. E. Baxter, Lewes.) 1s.

The Astronomical Clue to the Age of the Stone Implements in Kent's Cavern, 1919. (Journal of Torquay Natural History Society), pamphlet by R. A. Marriott.

The Ice Age Question Solved, 1919. Reprint from *Science Progress*, pamphlet by R. A. Marriott.

Warmer Winters and the Earth's Tilt; the Ice Age fully explained, by R. A. Marriott, 1921. (William Pollard and Co. Ltd., Exeter, price 1s.)

The Glacial Period and Drayson's Hypothesis, by John Millis, Col. Engineer Corps, U.S. Army, pamphlet reprinted from *Popular Astronomy* for December, 1921.

Unfavourable reviews of Drayson's works will be found in *Nature*, vol. vii, p. 301; vol. xi, p. 66; vol. lxxx, p. 71; vol. xciii, p. 108; the *Observatory*, No. 438, p. 320. *Knowledge*, vol. xii (New Series), p. 220 (230). They will be found excellent training. *Brûler n'est pas répondre.*

*In this book (page 174) was given for the first time the reason for the so-called 'retrograde' motion of the satellites of Uranus, since when this description has been quietly abandoned: compare for example Herschel's *Outlines*, Third Edition, 1850, par. 552, with modern works, *e.g.*, J. E. Gore's *Planetary and Stellar Studies*, page 78, *Everyman Encyclopædia* (Uranus), etc.

V.

LETTERS FROM *SCIENCE PROGRESS* QUARTERLY.

(*Referred to in pp.* 19, 21.)

(*From issue of July*, 1920, *p.* 101)—

The Ice-Age Question.

Dear Sir,—I am looking forward to the answer of astronomers to the question propounded in Major Marriott's very temperate reply to Mr. H. Spencer Jones's letter in the July number.* It seems hardly likely I am alone in this, for the present period is one in which 'authority' is viewed with disfavour except in so far as it justifies its position through the appeal to reason: this is markedly so in matters social and political, and still more should it be so in matters scientific. Therefore it is to be hoped that either Mr. Jones or some other representative of Greenwich has been prevailed upon to explain why Drayson's discovery—no inverted commas, please—has been left uninvestigated by official astronomy for close on half a century.

The geological facts and arguments adduced by Major Marriott are not, it would seem, contested by geologists and therefore stand. Yet official astronomy, while offering no explanation itself, rejects that which he gives; assigns no grounds for so doing; and seems content to reflect that the two sciences are at variance on another point. This is unsatisfactory, and indeed unreasonable. However, by their rejection of Drayson's problem astronomers have involved themselves in a very singular dilemma, as I will briefly explain.

There is a point in the heavens from which all the stars appear to be separating, those on the one side increasing their right ascensions and those on the other decreasing them. This spot is known as the Apex of Solar Motion, since it is thought to be the direction taken by the solar system in its motion through space. Its exact location is not definitely known, and many different determinations have been made during the last hundred years or so. Just about fifty years ago Drayson demonstrated that this spot was in—

R. A. 265·1 N. Dec. 31·1

The latest determination by Weersma in 1908 assigns it to—

R. A. 268·0 N. Dec. 31·4.

It will be seen that these positions are almost identical. They are at any rate vastly more accordant than any two that can be picked out of the list of thirty-six different determinations given by Campbell in his *Stellar Motions*.

Weersma's result is derived from all the 'proper motions' that were available to him in 1908 and is presumably to be preferred above any of the earlier ones cited by Campbell. Yet fifty years earlier Drayson secured an almost identical result—not by guessing, but by calculation—and without having recourse to the 'proper motion' of one single star! †

Essentially, both these determinations rest upon certain definitely ascertained facts, namely the recorded observations of stars and the discrepancy existing between the observed positions and the predicted positions as derived by calculation from the orthodox precession theory. But whereas in finding an explanation to fit the facts orthodox astronomy throws the effect upon the stars generally, and on the solar system as a whole, predicating heterogeneous motions in many vast and distant bodies, Drayson on the other hand explains the same phenomena—and, be it noted, reaches the same numerical result—by throwing the effect upon one homogeneous movement of a single small and near body, the earth.

The first of Newton's three *regulae philosophandi* requires that no more causes are to be admitted than are sufficient to explain the phenomena. Official astronomy is consequently confronted with this dilemma: that she must either admit Drayson's explanation, or palpably fly in the face of the First Rule in Philosophy.

* See Chronological Note at end (p. 41.)

† Since this was first printed I find that on reducing Drayson's value to the *same epoch* as that for which Weersma's is calculated, namely 1900.0, the accordance is actually closer than here appears, to wit, 267°·4, 31°·2.—A.H.B.

I trust you may be able to publish this letter, for the matter is too important to be let drop, and should be threshed out. Other points might have been enumerated, but the one given is that which I personally consider the most striking as *prima facie* evidence on the astronomical side of the question.

Yours faithfully,
ALFRED H. BARLEY.

(*From issue of October*, 1920, *p.* 266)—

COINCIDENCE, OR CONFIRMATION ?

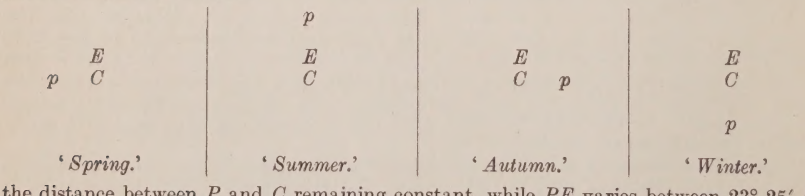
A Discovery relating to Drayson's centre of Polar Motion.

Dear Sir,— A letter appeared in the July number of *Science Progress* pointing out the fact that Drayson calculated, half a century ago, a position for the Apex of Solar Motion which agrees more closely with the latest determination (1908) than the latter does with any other; and the inference was drawn, that this coincidence was due to sound astronomy and not to chance.

Since that letter was in type another coincidence has been brought to light, quite accidentally, in the course of calculations made more or less out of curiosity. Before relating how the discovery came about, it may be useful to point out that in questions of evidence—circumstantial evidence that is—(and all vital matters concerning geological or archæological questions really do rest on circumstantial evidence)—in all questions of circumstantial evidence where a number of items are brought forward any one of which is liable to dismissal as mere coincidence not causally connected with the point at issue, it is not enough for Opposing Counsel to depreciate, *seriatim*, each separate item on this ground, and then claim a judgment in his favour because of the 'poverty' of the evidence. The *co-existence of the coincidences* is itself a piece of evidence, one that requires proper examination and should be given due weight. The presence of a small boy in a pantry may be a coincidence, his chin being smeared with jam may be another coincidence, the parchment of the jampot being broken may be a third, but no housekeeper would be blind to their collective significance; and in like manner the cumulative value of 'sidelights' of evidence (severally of small importance, perhaps) gathers both mass and momentum from each addition.

Now to the subject of the present letter. According to Drayson, the Pole of the Heavens, *P*, traces a circle in the sky round a point *C*, the radius of this circle being 29° 25′ 47″ and the period of revolution 31,756 years. The point *C* is situated 6° 0′ 0″ from *E*, the Pole of the Ecliptic, and *P*, *E*, and *C*, will be in alignment in A.D. 2295. These are the essential details of his discovery.

On pondering this brief statement it will be seen that the cycle of 31,756 years reproduces on a grand scale the main features of the common year; it has a 'summer' and a 'winter,' a 'spring' and an 'autumn'—'summer' in A.D. 2295 when the distance *PE* representing the obliquity is least (23°·4), and 'winter' A.D. 18172 (or 13583 B.C.) when *PE* is 35°·4, twelve degrees greater. The four quarters of this Great Year may be shown graphically thus:—



the distance between *P* and *C* remaining constant, while *PE* varies between 23° 25′ 47″ and 35° 25′ 47″.

The conditions which may be presumed to have prevailed during the 'winter' have been ably described by Major R. A. Marriott in his article "The Ice Age Question Solved" (*Science Progress* for April, 1919), while those of the 'summer' must be very like those of the present era. It occurred to the present writer that it would be interesting to examine the 'spring' conditions, and he therefore calculated the obliquity at that epoch when *PC* and *EC* are at right angles to each other. (This is a simple problem; *PC* and

CE being given in a right-angled spherical triangle, to find *PE*: cos *PC* × cos *CE* = cos *PE*.)

What was his amazement on finding it worked out to

29° 58′ 53″·0 ! !

The reason for this amazement may not be obvious to the reader, unless he happens to remember that the Great Pyramid of Egypt, the most stupendous astronomical monument of antiquity, is situated in North Latitude

29° 58′ 51″ (Piazzi Smyth).

From which it follows that, at this epoch (5644 B.C.), the Midsummer Sun must have culminated exactly over the centre of the Great Pyramid. The exactness of this accordance will be appreciated when it is remembered that the whole discrepancy, namely two seconds of arc, when reduced to linear measure, is only 202 feet, whereas the side of the Pyramid itself is about 750 feet, or nearly four times as great.

It is difficult to attribute this coincidence to chance. It looks uncommonly like design; for if the ancient architect of the Pyramid wished to erect a *gnomon* by which to determine the chronometry of the Great Year, he surely would relate it to one of the cardinal points of the cycle, 'spring,' 'summer,' 'autumn' or 'winter'; and of these points in the common year, spring is usually that upon which the thoughts of mankind—of poets and idealists at any rate—are focussed. This assumption of course presupposes on the part of the architect a knowledge of the length and other details of the cycle. Yet it also affords criteria by which the feasibility of such an assumption may be tested; for surely, in that case, the *gnomon* should be so constructed as to indicate not spring only, but other crucial points in the grand cycle or Great Year. And the question may be asked, Can it do this?

Let us see. In the common year, March 21st is referred to as the 'commencement' of spring, but June 21st is referred to as midsummer, not as the 'commencement' of summer: from which it would seem that March 21st would more fitly be termed mid-spring, and that February 4th when the sun is half-way between winter and spring should be regarded as the 'commencing' of spring. Certainly in the Great Year whose phenomena we are considering, the 'commencement,' in this sense, of 'spring'—that is to say, the point in the cycle at which the angle *PCE* is 135°—must have been an era anticipated by prehistoric (not preintellectual) mankind only less eagerly than spring itself. And the question arises, Could the Pyramid say when this point had been reached?

When the angle *PCE* is 135° the distance *PE*, or in other words the obliquity of the ecliptic or the sun's extreme declination in winter, would be 33° 24′ 1″·3, and calculating its altitude on December 21st we have:—

Altitude of the pole in lat. 29° 58′ 51″	29° 58′ 51″
Polar distance of sun	90° 0′ 0″
	33° 24′ 1″·3
Gives for angular distance from N. point of horizon	153° 22′ 52″·3
Take from	180° 0′ 0″
Gives altitude of sun at midday, December 21st	26° 37′ 7″·7

Now there are three long passages in the Great Pyramid, of which one points northward and two southward; the inclination of these passages is approximately equal, and Piazzi Smyth concludes the angle of inclination originally to have been, in all cases,

26° 18′ 10″.

However, as Colonel Howard Vyse in his earlier measurements gave 26° 41′ for the northern passage, it would seem (assuming all three had originally the same inclination) that 26° 37′ 7″·7 is not extravagantly wide of the mark. In any case, the accordance is near enough to be striking, and *more than near enough* to be practicable. For in spite of its length of 109 plus 156 feet, the southward-pointing passage, having a height of 47 inches, permits of a variation of plus-minus 2° 3′ in the direction of vision when using the passage as an instrument of astronomical observation. That is to say, altitudes ranging between 24° 15′ and 28° 21′ could be taken by using suitable 'sights'; and this, too, even if we accept the smaller value of 26° 18′ 10″ (Piazzi Smyth).

The last sentence prompts the reflection that the 'mid-winter' point could likewise be observed; for the Sun's south declination on December 21st would then be 35° 25′ 47″, whence altitude equals 24° 35′ 22″, which is within the range given.

To summarise briefly. It has been shown that, assuming Drayson's cycle to be a fact in nature:—

(i). The Great Pyramid is so situated as to have the Sun exactly—*exactly*—in its zenith at the epochs of 'spring' and 'autumn' in the Great Year.

(ii). A southward-pointing passage, of great length, exists in the Pyramid, and is inclined at such an angle as to render possible a minutely accurate observation of the Sun's altitude, sufficient to determine just when those points in the cycle corresponding in the common year to December 21st (mid-winter) and February 4th ('commencement of spring') had been reached.

(iii). These facts, if not mere coincidences, certainly appear co-related. And *if* they are only coincidences, then, as the reader has been reminded in the first paragraph of this letter, they are not isolated coincidences; for there is another, independent of them, which also demands explanation.

It is true that all this implies an antiquity for the Great Pyramid greatly exceeding that usually attributed to it. But does not Herodotus state that the Egyptian priests claimed that their archives recorded astronomical observations extending over more than three cycles of precession; and are we altogether justified in an *a priori* disbelief in such a claim?

One more point may be mentioned. The *north-pointing* passage in the Pyramid, as has been stated, is given an inclination of 26° 41′ by Colonel Howard Vyse; and since the point of the heavens thus exposed to view is 3° 18′ below the pole, it has been argued with great confidence by various writers on the archæology of the Pyramid, that it dates from the time when—by the orthodox motion of the pole—the fixed star Alpha Draconis (mag. 3·5) was at this distance from the pole: and a choice of two dates is possible, (i) when this star is approaching, and (ii) when it is receding from the pole. Obviously this theory requires the compound assumption that (*a*) this inclination of the passage was casual and not intentional on the part of the architect, and (*b*) that the motion of the pole was not known in those days (otherwise how account for their erecting a piece of architecture of such astonishing quality of endurance, when a few centuries would suffice to make it astronomically out-of-truth, or at least out-of-date?).

But if we assume the Drayson movement to be that of Nature, the argument for design and knowledge, as against ignorance and chance, becomes much stronger. For on calculating the *minimum* distance of Alpha Draconis from the pole according to this motion, one finds it to be just 3° 18′, the quantity required! Furthermore, the angle *PCE* is found to be 59° 45′, which is sufficiently near 60° to be remarkable, as we shall see (indeed if we make it exactly 60° the change in the polar distance *aP* thereby occasioned is imperceptible, and the difference in time is only twenty-two years).

We see, then, that the Great Pyramid, with its south and north-pointing passages, furnishes mankind with an enduring astronomical transit-instrument whereby the stages of the cycle represented by *PCE* assuming successively the angles, 180°, 135°, 90°, 60°, can be determined by solar and stellar observations with great precision. These positions in the cycle or Great Year correspond, it will be remembered, to the dates—

Dec. 21	Feb. 4	March 21	April 21
Mid-winter	*Commencement of spring*	*Mid-spring*	*May-time*

in the ordinary year, and their significance as seasonal points is obvious. May not the Great Year, perhaps, have a similar significance as regards the flowering of that hardy perennial, MANKIND?

July 1st, 1920. (Signed) Alfred H. Barley.

P.S.—*Apropos* the last sentence, since writing this letter I have come across this curious passage in a work dealing with the Evolution of Symbolism:—'They (the builders of the Pyramid) had it [that is, astronomical knowledge] assuredly; and it is on this knowledge that the programme of the Mysteries and of the series of Initiations was based; hence, the construction of the Pyramid, the everlasting record and indestructible symbol of these Mysteries and Initiations on Earth, as the courses of the stars are in Heaven.

The cycle of Initiation was a reproduction in miniature of that great series of cosmic changes to which astronomers have given the name of the Tropical or Sidereal Year [*i.e.*, the precession cycle] . . . . . Moses, an Initiate into the Egyptian Mystagogy, based the religious mysteries of the new nation which he created upon the same abstract formulae derived from this sidereal cycle. . . .'

Note.—The 'south-pointing' passage in the Pyramid, whose inclination I give as 26° 18′, is evidently that known as the Grand Gallery; but this does not extend upwards to the south face. A south-pointing passage which does so extend, and is often represented in Pyramid diagrams as parallel with the Grand Gallery, must have misled me into thinking the whole passage practically continuous. The conclusions in (ii) of my summary must therefore be maintained with reserve until further details of this passage are available. But they could be abandoned altogether without impeaching the main points referred to in (i) and (iii).—A.H.B.

Chronological Note relative to the Ice-Age Correspondence in "Science Progress," 1919—1921.

In *April*, 1919, Major R. A. Marriott, whose paper on the 'Downs and the Escarpments of the Weald'* had appeared in the same publication a year previously, contributed an article entitled 'The Ice-Age Question Solved.' In the following issue, *July*, 1919, a short letter appeared from Mr. H. Spencer Jones, now Chief Assistant at Greenwich Observatory, in which he said:—

'For the benefit of those of your readers who do not claim to be able to express 'an authoritative opinion on astronomical matters, I think it is desirable to point out 'that astronomers generally do not accept Drayson's theory. Into the reasons for its 'unanimous rejection it is not necessary to enter here. Any geological conclusions, 'however, which are based upon it should be received with caution. . . .'

In his reply in the next issue (*October* 1919) Major Marriott remarked that the evidence in favour of Drayson 'has now reached a stage which makes it less than ever "not 'necessary" for astronomers "to enter into the reasons for the unanimous rejection" 'of Drayson's theory,' adding that 'it is just these reasons that geologists and laymen 'wish to know. But if astronomers can answer this *one* question satisfactorily, it would 'meet the requirements of the case. If Drayson's reason for the continuous decrease 'of the obliquity is rejected, what takes its place?'

In the same issue Dr. W. H. Hatfield, of the Brown-Firth Research Laboratory at Sheffield, expressed regret at the absence of a discussion, and the opinion that it was 'hardly courteous, and certainly not truly scientific, for anyone to dismiss the matter 'so lightly as Mr. Jones has done,' and that 'readers have a right to expect such con-'tributors not only to say that they disagree, but also to give their reasons for their 'point of view.'

The succeeding issues for *January* and *April* 1920 contained no reference to the subject, and the letter in *July*, here reprinted, had evidently been held over some time. The same issue contained a two-page letter from Professor G. W. Tyrrell, A.R.C.Sc., F.G.S., generally criticising his paper. [This letter, with Major Marriott's reply, which appeared in *October* 1920 alongside Mr. Barley's second letter, are reprinted in full in *Warmer Winters* together with Mr. Spencer Jones's and Dr. Hatfield's letters (pp. 32-44).]

Unfortunately that terminated the correspondence, for on presenting some further archæological evidence Major Marriott was informed that it had been decided to close the discussion (*Warmer Winters*, p. 28).

While regretting this, one cannot but feel grateful to the Editor for the large amount of space (very precious in a Review devoted to every branch of science) which he accorded to this subject, and wonder that advantage was not taken earlier to reply on the astronomical side. But from some cause Greenwich maintained silence, and now at last has only spoken vicariously, as it were, in the person of a Member of the Council of the British Astronomical Association.

* So important to prehistorians as indicating the provenance and age of the North Downs Eoliths.

VI.

ASTRONOMICAL PROOF OF A RECENT ICE AGE.*

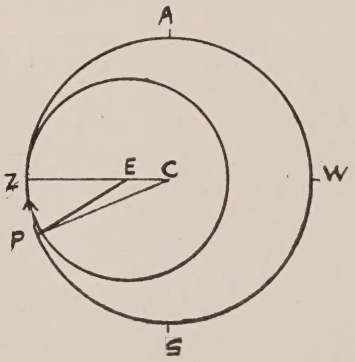
In approaching questions of climate, as in other matters, it is natural to reason from the known to the unknown.

Everybody knows that in summer the sun rises higher than in winter. And those that have travelled much also know that, for a given season of the year, it is hottest at those places where the sun is highest, and coldest where it is lowest.

It seems natural, then, to apply the same criterion when instead of travelling from one place to another place at the *present epoch*, we invert the process and attempt to travel from one epoch to another epoch while remaining at the *present place* ; thus for instance suppose we go back in imagination twenty thousand years and ask ourselves what was the climate of Hull then, was it glacial ?

If in attempting to answer this question we could say, definitely, "Well, at that date the sun on Christmas Day scarcely appeared above the horizon here (Hull), while at Newcastle he could not be seen at all," we should surely conclude that this country was then in the grip of an ice age, and that its climate can have differed very little from that of Nova Zemblia at the present time. This will be agreed.

As we *are* now able to say 'Yes' to this question, quite definitely, I propose to show the grounds on which that affirmative answer can be given.



If you will look at the little diagram you will see two circles, and on the left hand side the letter *P*. This represents the *present* position of the pole of the heavens, and the arrowhead shows the direction of its motion due to what is known as precession. For long it was supposed that this movement took place along the circumference of the smaller of the two circles, but the result of investigation has shown that it actually takes place around the larger circle.

Let us then consider what this means.

The thick line *PE* shows the distance between the pole of the heavens *P* and the pole of the ecliptic *E*. This distance is the measure of the extent of the tropics, and of the arctic circles, and as you know it is at the present time $23\frac{1}{2}$ degrees.

Now we can see at once from the diagram that *P E* is very near its *minimum* value, which it will reach at *Z*, after which it will increase, until when it reaches its maximum at *W* it will be greater than at present by about twice *C E* or 12 degrees

From this, then, it follows that not only in the future, but also in the past, the whole

* This was originally written with the aim of having it read, if possible, at the recent Meeting of the British Association for the Advancement of Science which was held at Hull. Being sent in too late, however, it could not be read and was accordingly distributed in the form of a leaflet at the discussion on "The Relation of Early Man to Phases of the Ice Age in Britain : (see *Eastern Morning News* for September 12th, 1922).

of Northern England will be and has been within the confines of the Arctic Circle and has experienced the concomitants of an Arctic climate. For if *PE* is increased 12° that means the Arctic Circle which is now in 66½ of latitude will come down to 54½ of latitude or about half way between Hull and Newcastle.

Study this little diagram, and you will find represented in it all the essential features of our familiar climatic cycle, the *year*, though on a vaster scale.

We are now in its summer, represented by *Z*. Some thousands of years hence, at *A*, we shall have arrived at its autumn ; some thousands later, at *W*, its winter, —— when, indeed, the human race will have cause to look forward to the approaching spring at *S*. This brief epitome must suffice for the moment. You can elaborate the details for yourselves later.

Now for the astronomical facts that underlie the foregoing statement and furnish proof that the larger circle is indeed the path described by the pole.

There are several but I will take one the reasoning in which you can all appreciate, and the actua numerical result of which you can all work out for yourselves at home. I must commence with an apparent digression, but it is closely connected with the subject.

About 150 years ago Sir William Herschel, after carefully studying the recorded positions of 13 bright stars, observed that the discrepancies existing between their positions as calculated, and as revealed by observation, pointed to a common tendency among them. He found that they all appeared to be *diverging* from the same quarter of the heavens.

He suggested an explanation as remarkable as it was ingenious. The obvious explanation was, that the whole of this apparent divergence was due to actual motion of those stars in the directions indicated. But Herschel, remembering Newton's first rule in philosophy which teaches that no more causes should be admitted than are sufficient to explain the phenomena, conceived that the discrepancies aforementioned were less likely to be due to these 13 stars moving in 13 different directions, than to our own solar system itself moving in 1 direction, namely, towards that point from which those stars seemed to diverge. (For if you have ever been on board a vessel approaching a landing stage at night, you cannot have helped noticing how the lights *open out* as you approach, as if diverging from a common point—which is the point the vessel is making for.)

Herschel supposed that this was an instance of the same kind. He determined, roughly, the situation of this point, and since his day many fresh determinations have been made with increasing degrees of refinement and employing many more stars. It is generally referred to as the 'Apex of Solar Motion,' or, briefly, 'Solar Apex.'

Herschel's supposition was a very ingenious one. From 13 discrepancies he deduced a destination for our Solar System. He gave a *reason* for those discrepancies. And you must admit it was a good reason.

But as Shakespeare says, 'Good reasons must perforce give place to better.'*

Let me ask you a plain question Look at that diagram and tell me, if the path of the pole has hitherto incorrectly been thought to be the *smaller* circle and if all calculations have been made on that assumption, whereas its true path is the *larger* circle,——tell me, at what point in the latter will the discrepancy between observed or *true* position and calculated or *supposed* position, be the greatest ? Surely it will be at *W*, for at that point the outer circle is farthest from the corresponding point on the inner circle ?

If, then, I prove to you that this point is absolutely identical with the latest and best determination of the so-called Apex of Solar Motion—which, you will remember, is the point at which the observed position of stars is found to DEVIATE MOST from their calculated position,—if I prove this to you, you will not need to be told which of these two explanations Newton's rule requires you to prefer, the simple one you have just listened to, or the earlier one of Herschel.

The earlier explanation requires us to suppose that the discrepancies alluded to are caused by the movement of many stars in all directions, our sun being one of them ; the other explains the same facts, in a quantitative manner, by reducing the erroneous

* "Julius Cæsar," iv, 3.

path of the pole to the true one; that is to say, by referring them to an unrecognised movement of our astronomical *sighting instrument*—THE EARTH.

Consider a moment this illustration. If a photographer having taking a flashlight photograph of this assembly afterwards exhibited a picture showing each person in two positions, one in his own seat and the other in his neighbour's, which explanation would you prefer?—that each member of the audience had simultaneously taken a leap either to the left or to the right, or, that the effect was due to an unnoticed movement of the camera itself?

In astronomy, the Earth is our camera; and it is therefore necessary to know with certainty all its movements, or our results will be vitiated.

Now for the numerical working out. The position of the Solar Apex will be found in Professor W. W. Campbell's *Stellar Motions* (p. 142). The most recent determination, made in 1908, and employing all the data then available, gives the position as in

Right Ascension	*268°·0*
North Polar Distance	*58°·6*

It is required to show that the point marked *W* on the outer circle is in this same spot. The procedure is very simple. Produce *E C* to *W*, and join *W P*. We then have the spherical triangle *P E W*, in which

P E	=	23° 27′
E W	=	35° 26′
P E W	=	174° 28′

We first require to find the side *P W* which will give us the North Polar Distance, and next we require to find the angle *E P W* which, subtracted from 270° will give us the Right Ascension.

Formula for solving the triangle PEW

$$\cos e = \cos w . \cos p + \sin w . \sin p . \cos E$$
$$\sin P = \sin E . \sin p \div \sin e$$

Then $270° - P$ = R.A. of *W*

e = N.P.D. of *W*

According to received notation *p* is the side opposite to *P*, *e* the side opposite *E*, and so on: cos *E* is negative, remember.

The result you will find is substantially identical with the position already given.

Someone may say: "But this is no proof. You have given us three parts of a triangle, which may lead to the result you say: *you haven't told us how you get them!*"

This is a sound objection, yet I can meet it.

The only quantity I assert dogmatically is the quantity *C E* = 6°. Even this has been derived by the process of successive approximation; and the others all flow from it: *P E* is the present obliquity 23° 27′, and *P C* is such a quantity as in connection with the other two will give the *present* decrease in the obliquity which in 1850 was about 0″·47 annually and is now slightly less, or say 0″·41 in 1900.

From this you can yourselves compute the values of *E W* and *P E W* if you wish.

WEGENER'S THEORY.

I should like to add one remark on Wegener's Theory, which I understand is to be discussed.

Wegener's Theory is known to me only by a Press cutting of 1912. But according to this cutting, the theory depends for its verification on astronomical observation to show that, for instance, *Greenwich* (England) and *Cambridge* (Massachusetts) have separated during a quarter of a century about 205 feet. This distance represents astronomically about 2 seconds of arc, quite a small quantity.

The detection of such a quantity is vitiated by the fact that there exists an *unrecognised* movement of the zenith of Greenwich Observatory—as also of others—which renders such quantities undependable. Therefore physicists, before basing important deductions upon such observations, should enquire into the matter.

Having satisfied myself by personal investigation of the existence of this movement, I feel bound to mention it.

(*From the 'Yorkshire Post,' 14/9/1922.*)

WEGENER'S THEORY AND GREENWICH MERIDIAN.

Sir,—Your special correspondent's remark, at the foot of his first column on the British Association to-day, that the Oxford Professor's statement that all our records of longitude are so much waste paper 'made the geologists open their eyes with amazement' is probably true of all of us, (for, as he says, 'astronomy claims to be one of the exact sciences'); and it interested me particularly. For although I was not present at the morning meeting, when Wegener's Theory was discussed, I attended the afternoon meeting on 'The Relation of Man to the Ice Age,' and although astronomy was not mentioned in the actual discussion, the 'astronomical proof of a recent ice age' leaflet which was distributed contains an incidental reference to the very point mentioned by Professor Turner. And that is the reason of the present letter.

The author there says:—'Wegener's Theory depends for its verification on astronomical observations. . . . Now there is an unrecognised movement of the zenith of Greenwich (and all other observatories). . . . Having satisfied myself by personal investigation of the existence of this movement, I feel bound to mention this.'

This seems to me a very important statement, chiming in as it does with the Oxford astronomer's admission that our longitudes are 'so much waste paper.' For obviously the determination of longitudes depends upon the zenith of the place from which longitudes are reckoned; and if the zenith of Greenwich Observatory, which determines the Prime Meridian for the whole globe, is subject to an unrecognised movement, the existence of which is thus independently testified to by two separate investigators, it is a matter demanding serious enquiry. The quantities concerned may be small, but that does not render them negligible, so long as they remain incalculable.

September 12. Yours, etc., NEW MEMBER.

VII.

THE ALLEGED PROPER MOTION OF CAPELLA.

(*In view of the remarkable admission by Professor Turner at the discussion on Wegener's Theory at the British Association Meeting at Hull in September last, that all our longitude determinations are "so much waste paper" when minute accuracy is in question, it seems advisable to print some of the concluding paragraphs of the writer's investigations referred to on pages* xxii, 18, 25 *and* 28.)

(p. 181 of MS.). 'We have then this result. The polar motion defined by Drayson shows the position of the star Capella to be reproduced with a fidelity that differs from the most dependable results of observation by only:—

$1''$ in N.P.D. over a period of 165 years.
$0^s{\cdot}08$ „ R.A. „ „ „ „

quantities minute in themselves, and of the same order of magnitude.

'This is a result which could not be excelled—hardly equalled—with the aid of the whole armoury of modern resources, that is to say, the latest adopted value for the so-called precession constant, and the best attested value of the alleged proper motion; these being deduced from a comparison of *innumerable observations*, and NOT, as this has been, from *a single dependable observation* and *three constants*.' [For actual comparison see next page.]

(p. 187 of MS.). 'And now to our outline review.

'1. The motion of Capella, as revealed by repeated observations, does not agree with the theoretical motion of the pole of the heavens round the pole of the ecliptic as a centre at a rate (derived from observation) of some $50''$ a year: about $0''{\cdot}43$ in declination and $0''{\cdot}135$ in right ascension being the annual discrepancy. To use Drayson's own dry remark, as the *theory* couldn't move, the stars had to, and accordingly this discrepancy is set down as the star's annual "proper motion."

'2. Multiplying the foregoing figures by 150 we find

65″ (dec.) and 20″ (r.a.)

as the discrepancy between observation and calculation by conventional theories when applied over a period of A Century and A Half. This demonstration [that is, the demonstration in MS. of which these are the concluding paragraphs] has shown that by the polar motion discovered by Drayson,—or rather, by the polar motion whose *true centre* Drayson discovered,—this discrepancy is reduced, by means which however complex in appearance all flow by the severest logical necessity from the one single centre C and the thence resulting quantities $C\,P$ and $C\,E$, and the Greenwich co-latitude $G\,P$, without straining or manipulation, this discrepancy is reduced to

1″ and 1″·2

only: a reduction of 6500 and 2000 per cent.

'3. The inference drawn from this is, that since it is in the last degree unlikely that a star pitched upon more or less by chance, among the many given by Drayson, and cited by him as an illustration of the complexities arising out of this polar motion in conjunction with the Zenith Movement*; and, moreover, investigated in a manner and by a method not prescribed by Drayson, and apparently (in some of its details at least) not known to him; since it is in the last degree unlikely that these factors should *combine by sheer chance* so obligingly just to fill the bill, the inference drawn is that this investigation proves that Drayson was right in his main contention and was right, too, in his quantities $C\,P$ and $C\,E$: or, at the least, that these quantities are *sensibly* correct.

'4. Should there still linger any uncertainty in the reader's mind; or rather, for it is the more likely, should the sheer inertia of past thinking prove too obstinate to be at once overcome, the evidence derived from the position of the Solar Apex, calculated in 1873 by Drayson without reference to the proper motion of any star, and agreeing almost to absolute *identity* of position with that determined by Weersma from all the proper motions available to him in 1908,—should be weighed and considered.

'5. Should a feeling exist that Drayson may be right in general conception, but that the two quantities $C\,P$ and $C\,E$ cannot be considered as established quantitatively,† then the evidence furnished by the joint testimony—mark this, the *joint* testimony—of the Great Pyramid's position in N. Lat.

29° 58′ 51″

which is the hypotenuse of a spherical triangle having $C\,E$ 6° and $C\,P$ 29° 25′ 47″ for its sides, *and requiring* the one side to be diminished if the other is increased, whereas the conditions of the $C\,P\,E$ triangle are such, in relation to the present rate of decrease in the obliquity, that both sides must be increased or both diminished in order to satisfy that rate at the present time,—the above values of $C\,E$ and $C\,P$ being the only ones which will simultaneously satisfy BOTH conditions: this evidence should be considered.'

The result of an actual calculation to test the statement in paragraph (2) may be appended. The starting point was January 1, 1887, and positions were calculated for 1.1.1755 and 1.1.1920, that is 132 years back and 33 years ahead. A similar calculation was made from the British Association Catalogue for 1850 in the manner there prescribed; and another from the Greenwich Catalogue for 1880; (— 95 + 70 and — 125 + 40 years respectively). The results are tabulated below and show "calculated positions *minus* observed":—

		1755.		*1920.*	
		r.a.	*n.p.d.*	*r.a.*	*n.p.d.*
		″	″	″	″
1850	B.A.C.	— 6·6	— 4·61 (+ 0·44)	+ 4·65	— 6·03 (—0·98)
1880	Greenwich	— 1·35	+ 0·21	— 0·75	+ 0·07
1887	Drayson	+ 1·20	— 1·55	— 2·47	— 0·53

* This passage is important in connection with Professor Turner's announcement.

† This was for a considerable time the author's own view, until the discovery now referred to was made.

As the position in N.P.D. appears to be incorrect by some 5″ in the British Association Catalogue, I have allowed for this in the figures shown in brackets, though it seems strange that so large an error in a Fundamental Star could have escaped detection before printing. It is possible to reduce the Drayson discrepancies to even smaller dimensions by making certain assumptions, but I have thought better to let them stand as here shown. The algebraical sum shows the total discrepancy between calculation and observation, when spread over the whole period of 165 years, as 1″·27 in R.A. and 2″·08 in N.P.D.

VIII.

A PARALLEL.

(1.)

'To-day the new world of truths revealed to us by a glance at a star-chart of proper-motions is occupying the minds of many mathematicians, astronomers, and natural philosophers, all engrossed in the exact sciences; and the talents of all are being devoted to patient observations, experiments and calculations which may lead, *in the course of ages*, to as clear an understanding of the driftings of the countless stars as KEPLER and NEWTON shed upon the planetary wanderings in our relatively contemptible solar system.

'It must not be supposed, from what has now been said, that the chart of stellar proper-motions exhibits throughout, and at a glance, any systematic flow of stars. On the contrary, *the general aspect of the picture is confused.* But the great fact remains that we become immediately certain that there are groups of stars following a common route. And the inference is irresistible that *the confusion will disappear when its causes have been fathomed* . .

'Every night that we look upon the stars we can now in imagination regard them as we do the great flakes of snow in a storm, whirled and drifted by currents of wind. Then we begin to envy the outlook of those who will take our place, *centuries hence*, when these whirling, drifting movements shall have been assigned their place . . .'

(2.)

'In the Middle Ages, towards the commencement of the sixteenth century, the condition of astronomy was one of immeasurable confusion, into which the ceaseless efforts of industrious men had reduced it. And why? Because they were working on a *false basis.*

'The Copernican change of centre heralded a new concept of the Solar System. And not only an astronomical, but a philosophical and in the end a moral and social revolution resulted. For change in thought precedes change in action, and has many and ramifying results. Revelation and revolution are but as cause and effect. .

'May it not be just possible that the Draysonian change-of-centre presages a new concept of the Sidereal System, and all the door-opening in the worlds of thought that that implies?

'Let those who are inclined to reject the suggestion offhand, ask themselves how they would regard a medieval astronomer who refused to contemplate the possibility of the well-tried and scientifically-proved system of his day giving way to one which seemed to deny its fundamental conceptions? Yet it did. And simplicity resulted. Now we have again reached complexity. The time is ripe for another revelation.

'Imaginative? Perhaps. And yet from the mistakes of the past one may learn lessons for the future.'

NUMBER ONE is from an interesting article by Professor George Forbes, M.A., F.R.S., which appeared in *Chambers's Journal* for September 1922 under the title "Flowing Streams of Stars." The italics have been added.

NUMBER TWO, as the reader will recognise, is from the "Conclusion" of this Reply (p. 30).

The context of both passages should be referred to, and, in connection therewith, the remarks on page 19 also. Professor Forbes has written an admirable *History of Astronomy*, published by Watts and Co. in 1905.

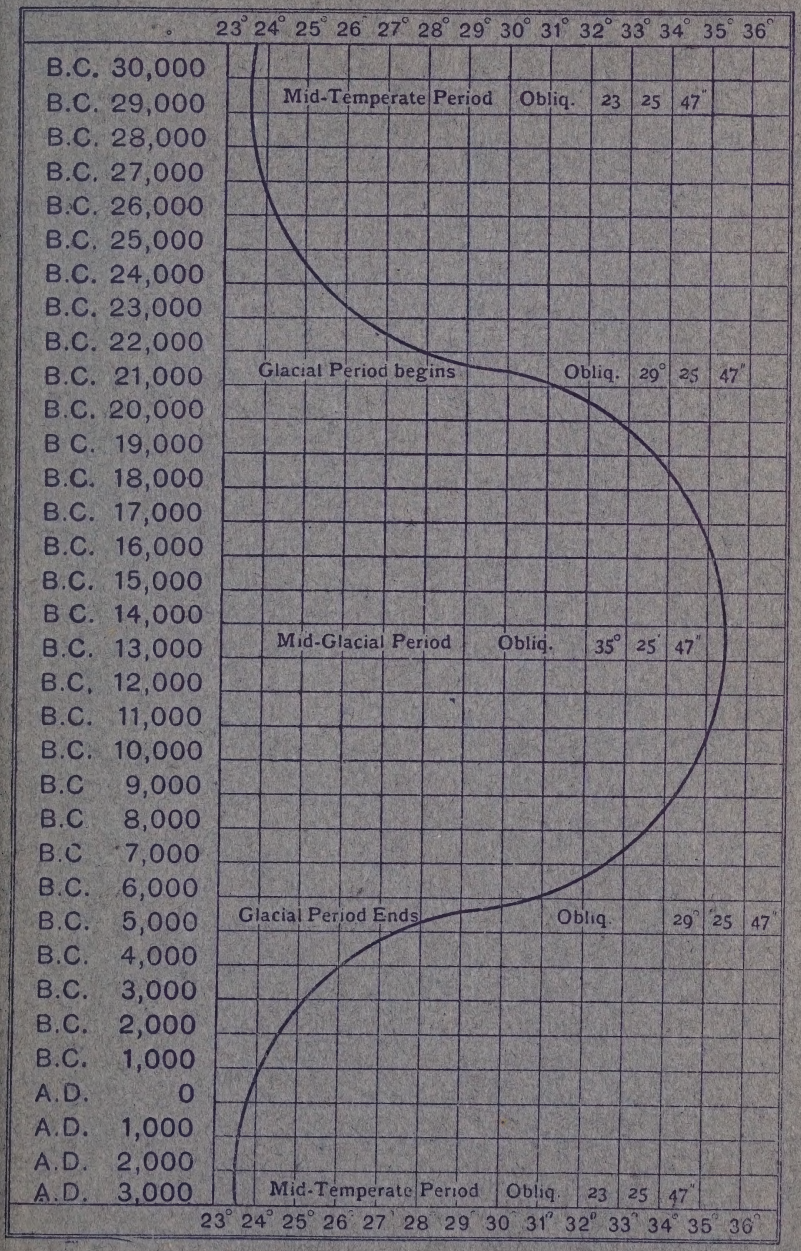
A COMPARATIVE SUMMARY.*

1. Drayson states, in opposition to astronomy, that the Glacial Periods were contemporaneous at both Poles.	1. The views held by astronomers regarding the Glacial Periods, will, on their own showing, have to be abandoned.
2. Drayson maintained that errors in sidereal time existed, which by his *data* could be corrected. In 1892 the error by Drayson would be 41s·28, according to de Horsey's calculation. [*See ante, p.* 19 and *W.W.*, p. 20.]	2. Mr. Stone, in 1892, then Radcliffe Observer at Oxford, working from a wholly different basis, asserted that sidereal time was incorrect by 41s·51.
3. Drayson can compute the obliquity of the ecliptic for any year, past or future, without observations.	3. This can only be done by astronomers by constant observations during the current year: [or, by a formula which shows the *rate* of decrease to be accelerating, whereas observation shows the contrary. See p. 24 and *W.W.*, p. 18.]
4. Drayson, by his *data*, can compute the Right Ascension and Declination of any star for any year, in the past or future, from one observation of that star.	4. Astronomers can, apparently, only do this from several observations at wide intervals, to arrive at a mean for application to the star in question. For an approximate forecast they are limited to a few years ahead.
Drayson's one movement—the annual motion of the Pole—called by him "the second rotation" of the earth, accounts for	(*a*) The progressive decrease in obliquity *and its rate* [italics mine—A.H.B.]. (*b*) The time and duration of the Ice Ages. (*c*) The changes in the co-latitudes of stars. (*d*) The error in sidereal time.
5. Drayson shows that the supposed movement of the entire solar system is only apparent since the effect is produced by the motion of the pole of the heavens towards the culminating point of maximum obliquity, the position of which accords closely with the mean of several astronomical observations to ascertain *the apex of solar motion.* [*See proof of this, pp.* 42-44.]	5. It is generally accepted by astronomers that the whole solar system is moving at a definite speed towards a point in the constellation Hercules, which is termed the "apex of solar motion."
6. Drayson shows that a complete explanation of the enigma of the acceleration of the moon's mean motion is furnished by adopting his centre of polar motion.	6. "Gravitation alone is incompetent to explain completely the observed motion of the moon." "Astronomy is at a standstill for an explanation."
7. Drayson, giving precise dates, states that the last glaciation began 23,700 B.C., and came to an end in 5,624 B.C.	7. Croll's theory, which has the support of astronomers [or *had*], states that the Glacial Period began 240,000 years ago, and came to an end 80,000 years ago. From evidence in America and Sweden [and elsewhere more recently, see *Warmer Winters*], geologists reckon that it passed away some 7,000 years ago, and agree generally with Professor Prestwich's opinion that it only lasted some 25,000 years in all.

[* *From Marriott's "Change in the Climate." I have investigated with some thoroughness* EACH ONE *of these claims; and, extraordinary though they appear—singly, and still more when taken together,—have satisfied myself that they are justified, and can be established.*]

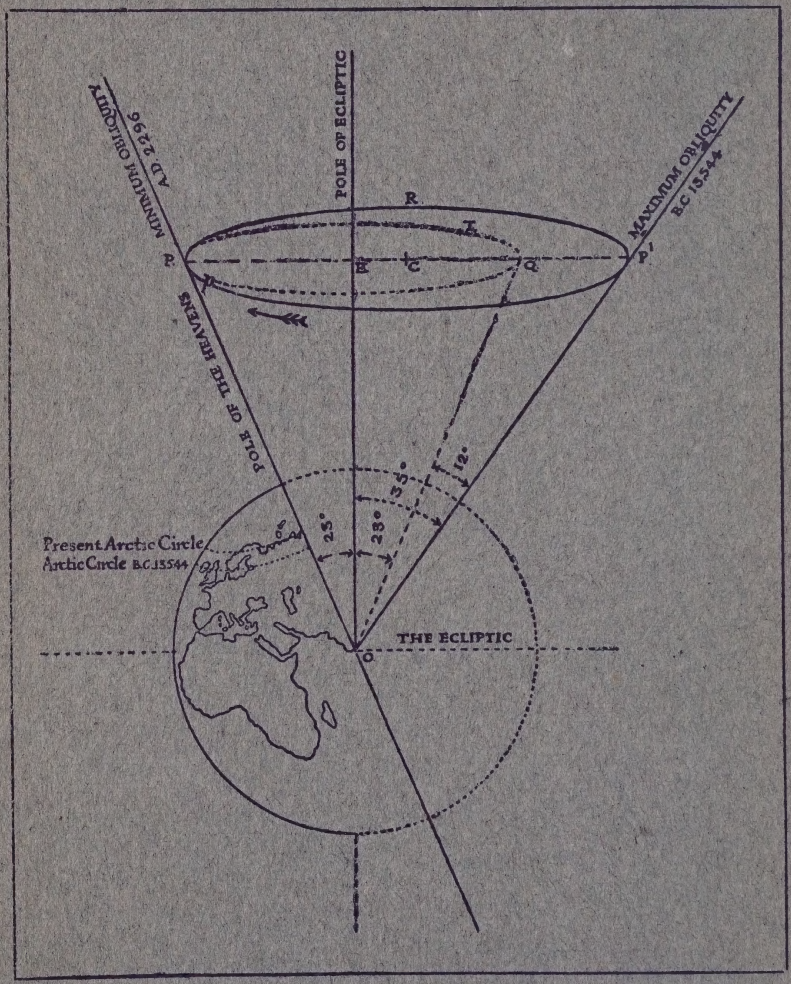
THE OBLIQUITY CURVE DURING A CYCLE OF 31,756 YEARS.

Shewing the commencement, duration, and termination of last Glacial Period.



Reproduced from *Draysonia* by kind permission of Admiral Sir Algernon F. R. de Horsey.

DIAGRAM OF THE SLOW CONICAL MOTION OF THE EARTH'S AXIS WHICH CAUSES THE PRECESSION OF THE EQUINOXES.



The dotted circle (in perspective) shows the path of the pole of the heavens, according to the usual astronomical theory. The revolution is supposed to take 25,868 years.

The outer circle *PRP*^{1} shows the path of the pole as worked out by Drayson, and is completed in 31,756 years. Obliquity of ecliptic at its maximum 35° 25′ 47″, minimum 23° 25′ 47″. Present obliquity 23° 26′ 58″.

The climatic importance of this movement with the geological and archæological evidence that it has received is fully explained in "Warmer Winters." —W. Pollard & Co., Exeter; 1s. 2d. post free.

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Astron.
B.

Author Barley, Alfred H.

Title The Drayson problem.

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